PROCEEDINGS

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IX. *A new Theory of Terrestrial Magnetism.*

By Professors JOHN PERRY *and* W. E. AYRTON*.

IN the autumn of 1876, while experimenting on magnetic transparency, we designed an apparatus for testing whether a moving body having a definite electric charge would, like a current, deflect a magnet. While waiting for the conclusion of the rains, and the advent of the very dry season which accompanies a Japanese winter, in order to try our instrument in conjunction with an ordinary plate-glass electrical machine, we received the account, published in the Philosophical Magazine for September 1876, of the experiments just performed by Mr. Rowland in the laboratory of Professor Helmholtz, by which it had been conclusively shown that a charge of electricity mechanically moved had the properties of an ordinary electric current as far as the deflection of a magnet was concerned†.

* Read March 8.

† Additional confirmation has recently been given on this subject by the experiments described by Mr. Crookes in his paper on the "Illumination of the Lines of Molecular Pressure &c.," since he has shown that the stream of particles which is shot off from the negative terminal in a very perfect vacuum, and which produces the green phosphorescence, carries electricity with it and is deflected by a magnet. This may be regarded as a sort of converse experiment, since it proves that a magnet deflects a moving charge of electricity.

Until this point was settled, it was unlikely that attention would be directed to the electromagnetic effects that might arise from the rotation of a charged body like the earth. Shortly, however, after the execution of the experiments referred to we attempted (as described in our paper "On Rain Clouds and Atmospheric Electricity," which appeared in the Philosophical Magazine for March 1878) the solution of a new theory of terrestrial magnetism. This problem we have attacked in a variety of ways; and the following solution, to which we have at length been led, we beg to offer for the acceptance of the Physical Society.

The points near the surface of the earth have different linear velocities from those in the interior (although all the points have the same angular velocity of rotation round the earth's axis); therefore, if the earth had an initial electrical charge, residing of course, in accordance with the well-known electrical law, on its surface, the electrified particles would have velocities relative to the remainder; hence, as a direct consequence of the results of the experiments published by Professor Helmholtz, the interior of the earth would be a magnetic field, quite independently of its interior constitution. And precisely similar reasoning, of course, proves that outside the earth's surface there would also be a magnetic field. (*Vide* addition at the end of the paper.) To determine the strength of this field we have the following relationship to start with.

In § 526, Clerk Maxwell's 'Electricity,' it is shown that an element of current C, of length ST, acts upon a unit magnetic pole at a point P with a force

$$C \frac{ST}{PS^2} \sin PST,$$

in a direction at right angles to PS and ST. Combining this with the experiments referred to above, we may assume that if a charge of static electricity (measured in electromagnetic units) Q, at the point S, moves in a direction ST with a velocity *v relative* to a point P, it produces on a unit magnetic pole at P a force

$$\frac{Qv}{PS^2} \sin PST,$$

in a direction at right angles to PS and ST; and this is the *only assumption* employed in the following investigation.

Now suppose the earth to have the uniform density of electricity σ over its surface, and let its radius be unity. Consider the force produced, by the rotation of the electricity at a point S on the surface having coordinates r, θ, ϕ, at a point inside the sphere having the coordinates r_1, θ_1, ϕ_1. Then if the sphere be rotating with an angular velocity w round the axis of z, and if θ be the angle between this axis and a radius, while ϕ is the angle between the axis of x and the projection of a radius, the velocity of S relative to P will have for its coordinates

u or $-w(\sin\theta\sin\phi - r\sin\theta_1\sin\phi_1)$ parallel to x,

t or $w(\sin\theta\cos\phi - r\sin\theta_1\cos\phi_1)$ parallel to y;

also

$$PS^2 = l^2 + m^2 + n^2,$$

where

$$l = \sin\theta\cos\phi - r\sin\theta_1\cos\phi_1,$$
$$m = \sin\theta\sin\phi - r\sin\theta_1\sin\phi_1,$$
$$n = \cos\theta - r\cos\theta_1.$$

Now the direction-cosines of PS are proportional to l, m, and n; and the direction-cosines of ST, the direction of motion of S *relatively* to P, are proportional to u, v, or to $-wm$, wl, and 0. Consequently PS is perpendicular to ST. Also, if λ, μ, ν are the direction-cosines of a line at right angles to PS and ST,

$$\lambda = \frac{nl}{PS\sqrt{l^2+m^2}},$$
$$\mu = \frac{mn}{PS\sqrt{l^2+m^2}},$$
$$\nu = \frac{-(m^2+l^2)}{PS\sqrt{l^2+m^2}},$$

where the negative sign must be given to the root. Now if F is the force at P due to the charge of surface-density σ on the elementary area $d\theta \,.\, \sin\theta \,.\, d\phi$ at S moving relatively to P, we know that it must be at right angles to PS and ST, and equal to

$$\frac{\sigma \sin\theta \,.\, d\theta \,.\, d\phi \times \sin PST\sqrt{u^2+t^2}}{PS^2}.$$

Hence, if δX, δY, δZ be the resolved portions of the force F

parallel to the axes of coordinates, and if $d\mathrm{S}$ stands or $\sin\theta \,.\, d\theta \,.\, d\phi$, we have

$$\delta\mathrm{X} = \frac{\sigma \,.\, d\mathrm{S} \,.\, \sqrt{u^2+t^2}}{\mathrm{PS}^2}\lambda\,;$$

and similarly for $\delta\mathrm{Y}$, $\delta\mathrm{Z}$.

Hence, if $\iint$ stands for the summation over the whole surface of the sphere, and if X is the total force at P in the direction of the axis of x,

$$\mathrm{X} = -\iint \frac{\sigma\, d\mathrm{S}\, w\sqrt{l^2+m^2}}{\mathrm{PS}^2}\,\frac{nl}{\mathrm{PS}\sqrt{l^2+m^2}}$$

$$= -\iint \frac{\sigma w\, d\mathrm{S}}{\mathrm{PS}^2}\, n\,\frac{l}{\mathrm{PS}}.$$

Similarly,

$$\mathrm{Y} = -\iint \frac{\sigma w\, d\mathrm{S}}{\mathrm{PS}^2}\, n\,\frac{m}{\mathrm{PS}}$$

$$\mathrm{Z} = \iint \frac{\sigma w\, d\mathrm{S}}{\mathrm{PS}^2}\,\frac{m^2+l^2}{n}\,\frac{n}{\mathrm{PS}}.$$

Now the resolved part of the force along the axis of x is the same as would be the force in that direction due to a distribution of attracting matter of density $-n\sigma w$ over the surface of the sphere. Similarly the resolved part of the force along the axis of y is the same as would be the force in that direction due to a distribution of attracting matter of density $-n\sigma w$ over the surface of the sphere; and the force in the direction z is the same as would be the force in the same direction due to a distribution having a density $\sigma w\,\dfrac{l^2+m^2}{n}$ or $\sigma w\left(\dfrac{\mathrm{PS}^2}{n} - n\right)$.

Regarding the force as being due to such a distribution,

$$\mathrm{Z} = \iint \frac{\sigma w\, d\mathrm{S}}{\mathrm{PS}^2}\cdot\frac{\mathrm{PS}^2}{n}\cdot\frac{n}{\mathrm{PS}} - \iint \frac{\sigma w n\, d\mathrm{S}}{\mathrm{PS}^2}\cdot\frac{n}{\mathrm{PS}}.$$

Now the first integral is $\iint \dfrac{\sigma w\, d\mathrm{S}}{\mathrm{PS}}$, which we know has a value equal to the potential inside the sphere due to a uniform distribution of density σw over the surface, and is therefore a constant, $4\pi\sigma w$. So that the entire force in the direction z is $4\pi\sigma w$ minus the force in direction z due to a distribution of attracting matter of density $n\sigma w$ over the surface of the sphere.

Now it is easy to show that a distribution of attracting matter of a density proportional to n or to $A+C\cos\theta$ over the surface of a sphere will give

$$X=0,$$
$$Y=0,$$
$$Z=\text{ a constant};$$

therefore all that is necessary is to determine the value of this constant. We neglect the term A, because a uniform distribution produces a constant potential, or a zero force in all directions; the distribution $C\cos\theta$ being a zonal harmonic, produces a potential inside the earth,

$$V=\frac{4\pi C}{3}r\cos\theta_1,$$
$$=\frac{4\pi}{3}Cz;$$

so that the force Z which equals $\frac{dV}{dz}$ is $\frac{4\pi}{3}C$. Thus for the distribution $C\cos\theta$ we have the force $\frac{4\pi}{3}C$; so that the constant force above mentioned, $4\pi\sigma w$, requires the distribution $3\sigma w\cos\theta$. From this we must subtract the distribution $n\sigma w$, or $\sigma w(\cos\theta-r\cos\theta_1)$, giving us for the total distribution of attracting matter over the surface of the sphere a density

$$2\sigma w\cos\theta+\sigma wr\cos\theta_1;$$

but the latter term means a uniform distribution, producing therefore no internal force, and may therefore be neglected. And the first term is a zonal spherical surface harmonic; therefore the electromagnetic potential due to the rotation of the electricity on the surface of the earth is

$$\frac{4\pi}{3}2\sigma wr\cos\theta_1 \text{ inside the earth,}$$

and

$$\frac{4\pi}{3}2\sigma w\frac{1}{r^2}\cos\theta_1 \text{ outside the earth,}$$

where w is the angular velocity of the earth on its axis, r the radial distance of any point from the earth's centre, θ_1 the co-latitude of the place, $4\pi\sigma$ the total quantity of electricity uniformly distributed over the surface of the earth measured electromagnetically, and the unit of length the earth's radius.

These results, which we think are logical consequences of the experiment performed in Professor Helmholtz's laboratory, and referred to at the commencement of this paper, may now be applied in various ways.

For example, if the iron of the earth is arranged nearly in a hollow sphere, of external and internal radius a_2 and a_1, then, since the potential given above is a zonal harmonic, we can at once apply Poisson's result; and we find that the potential due to magnetization of the hollow sphere is

$$\frac{4\pi\kappa(3+8\pi\kappa)(a_2^3-a_1^3)\frac{8\pi}{3}\sigma w a_2^3}{9+36\pi\kappa+32\pi^2\kappa^2(a_2^3-a_1^3)}\frac{\cos\theta}{r^2}$$

for all points outside the outer surface of the sphere; and hence, for points outside the surface of the earth, the total magnetic potential is

$$V=\left[\frac{4\pi\kappa(3+8\pi\kappa)(a_2^3-a_1^3)\frac{8\pi}{3}\sigma w a_2^3}{9+36\pi\kappa+32\pi^2\kappa^2(a_2^3-a_1^3)}+\frac{8\pi\sigma w}{3}\right]\frac{\cos\theta}{r^2},$$

where κ is the coefficient of magnetization.

Now Biot's approximation to the law of intensity of the force is

$$\sqrt{1+3\sin^2\lambda},$$

where λ is the latitude of the place; and we understand that this approximation is generally considered, for rough purposes, as a fairly accurate one.

Our equation for any point at a distance r from the centre and having a colatitude θ is

$$V=M\frac{\cos\theta}{r^2};$$

and

$-\frac{dV}{d\theta}$ is the force directed towards the north,

$-\frac{dV}{dr}$ is the force directed downwards towards the earth's centre;

therefore, if I be the magnetic intensity,

$$I^2=\left(\frac{dV}{d\theta}\right)^2+\left(\frac{dV}{dr}\right)^2$$
$$=\frac{M^2\sin^2\theta}{r^4}+\frac{4M^2\cos^2\theta}{r^6},$$

or, putting r equal to unity (that is, for a point on the earth's surface),

$$I = M\sqrt{1+3\cos^2\theta}$$
$$= M\sqrt{1+3\sin^2\lambda},$$

which is Biot's expression.

Now this is a result which could not have been anticipated, and speaks well for our new theory of terrestrial magnetism.

It is well known that many forms of distribution of iron inside the earth may be found which, with the existing potential given above, will produce Gauss's distribution of potential over the surface of the earth; and it would be very interesting to try how close an approximation to the real potential would be obtained by considering the iron of the earth to form a hollow ellipsoid, one of its principal axes coinciding nearly, but not quite, with the earth's axis. This calculation would be comparatively easy; but we prefer at present merely confining ourselves to simple illustrations of our theory.

Thus, let us, for simplicity, assume that the magnetic matter of the earth is iron, with everywhere a coefficient of magnetization

$$\kappa = 30^*;$$

then the terms involving κ^2 will be large compared with the others; so that if, for a very rough approximation, we assume a_1 to be nought, and a_2 equal to unity, or the whole earth to consist of iron, we find

$$V = \frac{16\pi}{3}\sigma w\frac{\cos\theta}{r^2}.$$

Now Gauss gives for the magnetic moment of the earth,

$$3{\cdot}3092\,n^3,$$

in millimetre-milligramme-second units, and where n is the number of centimetres in the earth's radius. Consequently, since the dimensions of a magnetic moment are

$$M^{\frac{1}{2}}L^{\frac{5}{2}}T^{-1},$$

* κ for ordinary iron is probably between 20 and 30; and in our ignorance of the internal state of the earth, or what effect the great heat or pressure may have on the coefficient of magnetization, we are compelled to use this value; but it is possible, of course, that the real value of κ may be very different.

the earth's magnetic moment becomes

$$\cdot 00033092\, n^{-\frac{5}{2}} n^3,$$

the units being the earth's radius, gramme, second.

Assuming Biot's distribution of magnetic force over the surface of the earth, which is also what our theory has led us to, we then get from Gauss's expression for the moment the result that the magnetic potential on the earth is

$$0{\cdot}33092 \cos\theta\, n^{-\frac{1}{2}},$$

or

$$0{\cdot}00001311 \cos\theta \text{ nearly};$$

so that roughly we have, for a point on the earth's surface,

$$\frac{16\pi}{3}\sigma w = 0{\cdot}00001311.$$

But

$$w = \frac{2\pi}{24 \times 60 \times 60};$$

$\therefore$ the density

$$\sigma = 0{\cdot}0107 \text{ unit of electricity,}$$

or the total charge

$$= 4\pi \times 0{\cdot}0107,$$

the fundamental units of space, mass, time being the radius of the earth, the gramme, and the second. But the dimensions of σ are $M^{\frac{1}{2}} L^{\frac{1}{2}}$; so that, in order to express σ in C.G.S. units, we must multiply by $\sqrt{n}$; therefore the total charge

$$= 4\pi \times 0{\cdot}0107 \sqrt{\frac{2{,}000{,}000{,}000}{\pi}} \text{ C.G.S. units}$$

$$= 4\pi \times 0{\cdot}0107 \sqrt{\frac{2{,}000{,}000{,}000}{\pi}} \times 10^7 \text{ microfarads.}$$

To get an idea of the electromotive force required to produce this charge, let us imagine one pole of a Daniell's battery connected with the earth and the other with all bodies in space. Then, since the capacity of the earth is 630 microfarads, this charge will be produced for each cell so employed; so that, if x is the number of cells necessary to produce our required distribution,

$$x = \frac{4\pi \times 0{\cdot}0107 \times \sqrt{\frac{2 \times 10^9}{\pi}}\, 10^7}{630}$$

$$= 54 \text{ million roughly.}$$

We have therefore proved that if the earth be electrified, it must, from its very rotation, quite independently of all other bodies in the universe, be magnetic; and if it consist of a shell of iron, thick or thin, then that the law of distribution of magnetism produced by this electric charge in mechanical rotation will be identically that given by Biot; and, lastly, if the earth were wholly of iron, a difference of potentials of about fifty-four million volts between it and space would be sufficient to produce the necessary amount of charge.

Now, although fifty-four million volts is a large difference of potentials to be produced with a galvanic battery, there would not be the slightest difficulty in the earth having such a difference of potentials between it and space, seeing that the earth is surrounded by millions of miles of interplanetary vacuum, every inch of which is as good or better an insulator than a Crookes's vacuum; and it has been experimentally shown that many thousands of cells will not cause a discharge across even a comparatively thin film of such a vacuum.

But even without considering the highly insulating character of interplanetary space, we see from the experiments of Drs. De La Rue and Hugo Müller that the electromotive force of fifty-four million cells cannot, in all probability, initiate a spark between two points in ordinary air unless the distance separating them be less than four hundred feet. Consequently, if the electric charge which by its mechanical rotation produces the magnetism be on the earth itself and not in the air, it could not be discharged by sparking, unless another planet, having at least a potential nought relatively to the earth, came at least to within four hundred feet of its surface.

Next, as regards the sign of the electric charge on the earth's surface required to produce the earth's magnetic polarity, is it in accordance with the known phenomena of atmospheric electricity? To produce the earth's magnetism, we must have, in accordance with the known laws of electromagnetism, a *negative* current flowing from west to east, or in in the direction of rotation of the earth. In the language of the new theory, therefore, the surface of the earth must be *negatively* charged; but Sir William Thomson has proved, by observations with his electrometer, that *all* the phenomena brought to light by atmospheric electricity, on a fine day,

could be produced by the sole agency of the earth having a *negative* charge and without any charge in the air itself. The negative charge, therefore, required for our explanation of the cause of terrestrial magnetism is sufficient to account for all the ordinary phenomena of atmospheric electricity.

In the preceding investigation we have supposed the electric charge to be uniformly distributed over the earth, and so have arrived at a law of magnetic intensity merely varying with the latitude. But the sun and other members of the solar system may very likely have potentials so different from that of the earth that we can hardly conceive the amounts; consequently we should expect the static electric distribution of the earth would undergo periodic changes corresponding in time with those of the ocean-tides. But alteration in the static distribution of electricity on the earth's surface means, as we have shown, alteration in the law of magnetic intensity; consequently we should expect that this magnetic intensity would vary somewhat as do the ocean-tides; and this is known to be the case. But it is also evident that, besides these regular changes, every time a great mass of vapour is suddenly formed and condensed on the earth, and whenever great changes are occurring in the solar atmosphere whereby the lines of *electrostatic induction* from the sun to the earth are altered, we should find corresponding changes in terrestrial magnetism such as we now know as magnetic storms. And not only this, but as the planets are charged bodies, their motions relatively to the sun ought to cause motions in the sun's atmosphere such that, for instance, the allineation of a number of planets and the sun, or the near approach of any planets, if the alligned or approached planets have potentials nearer that of the sun than many of the other bodies of the solar system, ought to diminish the storms in the solar envelope, and ought to alter the electrostatic distribution on the earth. But it has been shown that the near approach of a planet to the sun both affects the sun's spots and terrestrial magnetism.

And, lastly, since the iron in the earth may, from its great pressure, possess great coercive force, we should expect (as we know to be the case) that magnetic changes would lag behind the astronomical influences accompanying them.

Addition, April 17th.—Since the reading of this paper before the Physical Society, several criticisms have appeared of this proposed explanation of terrestrial magnetism. Some of the writers have failed to realize that the various parts of a rotating sphere have relative motions one to the other, of such a nature that if some were electrified the others would become magnetized. Their difficulty seems to have arisen from the fact that the motion of a particle of a rotating rigid sphere consists of a rotation round the axis of the sphere combined with a rotation of the particle round its own axis; so that if two particles be looking at one another in one position of the sphere, they are looking at one another in all positions, just as the same side of the moon is always turned to the earth. But if this rotation of the particles round their own axes could be stopped, if, in fact, the motion of the particles became what is commonly known as "sun-and-planet motion" similar to that of the bobbins in the machinery used in sheathing telegraph-cables to prevent torsional strain being put into the iron wires as they are lapped on the core), then probably these writers would have no difficulty in seeing that the interior particles would be magnetized by the more rapidly moving electrified ones. Now the motion of an *un*electrified particle round its own axis cannot in any way prevent it becoming magnetized by electrified particles revolving round it; for if it could, it would be equivalent to saying that, if the electrified particles were at rest and the *un*electrified one revolving, the mere revolution of the latter would magnetize itself oppositely to the way it would be magnetized if it were at rest and the electrified ones only in motion—a result not only without experimental proof, but one also highly improbable. Consequently, if the particle has the two motions together (as it has in a rigid revolving sphere), it will still become magnetized if the surface of the sphere is electrified.

In fact, so little can the motion of a mass of iron prevent its becoming magnetized by a moving charge of electricity, that it has been suggested to us, within the last few days, by Mr. G. F. Fitzgerald, of Trinity College, Dublin, that probably a mass of iron would become magnetized by a static charge of electricity if both had rapid absolute motion in space, even although in the same direction in parallel lines and with

the same velocity—in fact, that *relative* motion is unnecessary. If this assumption (which has not yet been experimentally tested) be true, then not only will the mathematical analysis required in the investigation of the problem contained in this paper be much simplified, but, in addition the charge of electricity on the earth's surface necessary to produce, by its mechanical rotation, the earth's known magnetic moment will be considerably less even than the charge calculated above. We hope to put this idea of Mr. Fitzgerald's shortly to an experimental test; but in the meantime we give no further indication of the reasoning by which it has been arrived at, nor of the way in which our equations would be modified, preferring to leave the investigation in a perfectly rigid form as it now stands, rather than to introduce any assumption which might appear problematical, even although such an introduction would both add weight to our theory, and might explain, from the velocity *in space* of a place at midnight being greater than at midday, the cause of the solar-diurnal magnetic variation.

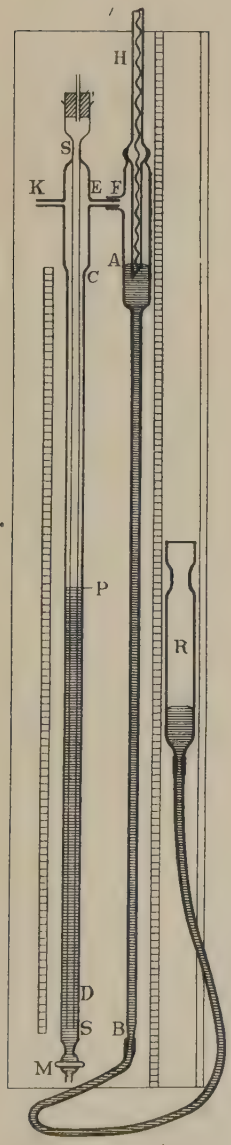
X. *The Maintenance of Constant Pressures and Temperatures.* *By* Frederick D. Brown, *B.Sc.*

[Plate IX.]

The great majority of the results obtained from physical experiments vary with the temperature at which the observations are made. The measurements of the density of a substance, for example, of its refractive index, of its electric conductivity, of its elasticity, of the maximum tension, and of the latent heat of its vapour, all require that the temperature should not vary during the observations. Hitherto many of these measurements have been confined to temperatures differing little from that of the atmosphere; such temperatures are easily maintained constant by means of a bath of water or other liquid; but when we try to make observations at higher temperatures, the means at our disposal fail us, and we find that, except at certain points (such as 100°), we cannot keep up the same temperature long enough to make at leisure accurate readings of our instruments.

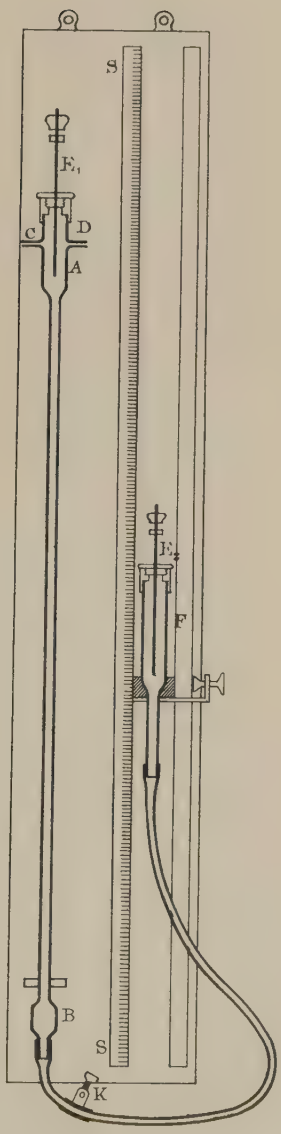
Many attempts have been made to obviate this difficulty,

Fig. 1.



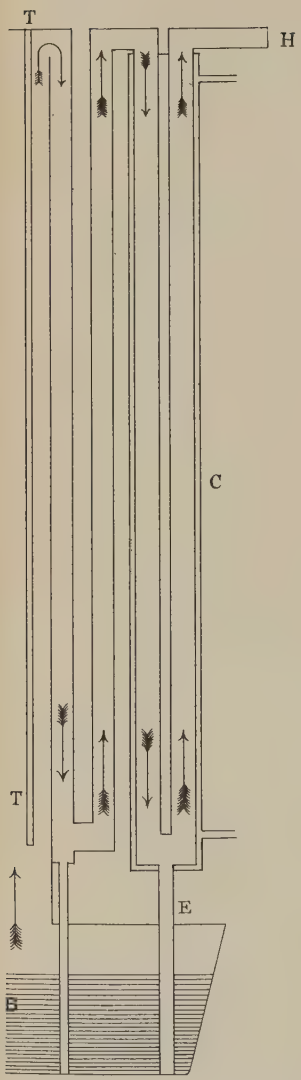
Scale $\frac{1}{7}$

Fig. 2.



Scale $\frac{1}{7}$

Fig. 6.



Scale $\frac{1}{7}$.

Fig. 4.

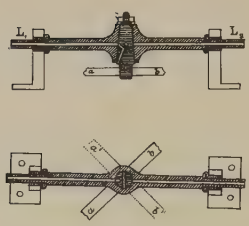


Fig. 5.

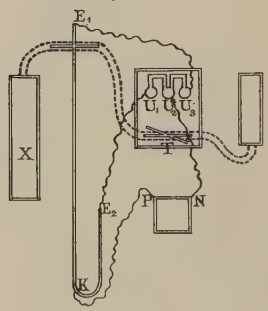
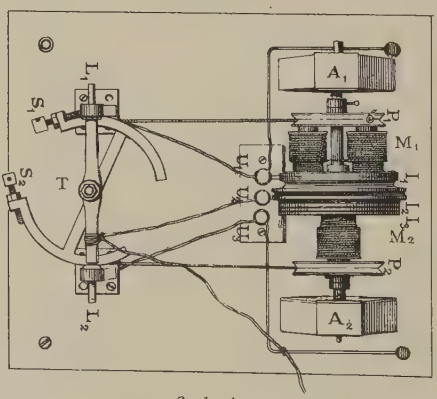


Fig. 3.



Scale $\frac{1}{6}$.

but, as it seems to me, without complete success. The ordinary method has been to use a large quantity of water or other liquid, and to keep it in continual agitation; above 50°, however, the temperature of such a bath is rarely, or never, rigorously constant, while the inconvenience and waste of time incurred in heating the large mass of liquid to the required point are by no means to be neglected.

In order to keep such a bath at a constant temperature, a large number of gas-regulators have been invented. In most of these a small vessel containing mercury or air is placed in the bath, and arrangements are made by which the gas-supply is partially shut off when the mercury expands beyond a certain point. Probably the most sensitive form of this kind of apparatus is that recently described by M. Benoit to the French Physical Society. Here a small closed vessel containing methyl acetate is placed in the water the temperature of which is to be maintained constant; this vessel communicates with a manometer containing mercury; and as the vapour-tension of the methyl acetate increases, the mercury in the manometer rises, obstructing the flow of gas to the burner in the usual way.

I have applied this form of thermostat to a bath of water used for heating a long column of mercury. The water is contained in a vertical copper cylinder 42 inches in length by 6 in diameter; the cylinder is packed with felt on the outside, and contains within it a second smaller and shorter one open at both ends, and extending to within about 2 inches of the top and bottom. By means of a suitable mechanical arrangement, the water is made to flow continuously down the space between the two cylinders and up the inner cylinder; the upper and lower portions of the bath are thus kept at exactly the same temperature. The water is heated by allowing it to circulate through a copper coil placed over a gas-burner. Even when the small vessel of the thermostat is filled with ether, the tension of which varies much more for a given variation of temperature than does that of methyl acetate, the temperature is subject to fluctuations of as much as 0°·1, and this independently of the change which necessarily occurs when the pressure of the gas is altered.

Gas-regulators are employed perhaps more successfully

where the gas-flame can be applied directly underneath the bath; for the rise of temperature in the water then follows more rapidly upon the increase of the flame, the flow of gas is sooner checked by the mercury, and the tendency to allow too much heat to be communicated to the water is thereby lessened. Still better results are obtained if the thermostat be applied to an outer jacket of water surrounding the bath in which the observations are made. Both these conditions, however, are generally very difficult to carry out where large quantities of water are of necessity used. Even when all these precautions are taken, a constant temperature, in the true meaning of the words, is not attained. For a further discussion of the defects of this form of thermostat see Laspeyres*.

The only practicable way of attaining the object in view appears to be afforded by the fact that the vapour emitted by a boiling liquid does not vary in temperature, provided that there is no variation either in the composition of the liquid or in the pressure to which it is subjected. Taking their stand on this consideration, Laspeyres (*loc. cit.*) and Sprengel (Journ. Chem. Soc. 1873, p. 458) suggested the use of mixtures of sulphuric acid and water of different strengths, which, if the vapour given off is condensed and returned to the mass, boil constantly at certain given temperatures: here the constant temperature is afforded by the liquid and not by the vapour, which is less hot; hence, if the liquid tends to become superheated, fluctuations in temperature will infallibly occur. The great objection to this method is that, when a series of temperatures is required (as, for instance, in the comparison of thermometers), great inconvenience and loss of time is incurred by having frequently to replace the liquid in the apparatus by another containing a different proportion of sulphuric acid or other substance.

A series of temperatures can be obtained with one liquid only, if the pressure under which it boils be varied. This method, simple as it appears, is beset with considerable mechanical difficulties, to surmount which the apparatus described below has been constructed.

At first sight it would seem that, if the vessel containing the steam be connected with a large closed vessel serving as air-

* Pogg. *Ann.* clii. p. 132.

reservoir, all that is necessary is to rarefy or compress the air in the reservoir to the required extent, and to allow the liquid to boil undisturbed. But the steam-bath is necessarily large; and the reservoir therefore must be large also. Now the reservoir must not only be strong enough to stand a vacuum inside it, but must also be capable of supporting an interior pressure of at least 50 lbs. on the square inch; such a reservoir is not only cumbrous and expensive, but dangerous. Further, any change of temperature in the room alters the pressure inside this reservoir; it must therefore be placed in a cistern of water, which is a second objection. The great obstacle to the employment of this method, however, is the difficulty of preventing leakage even with the greatest possible care. In endeavouring to stop leaks I expended so much time that I gave up the reservoir, and determined to construct an apparatus for maintaining a constant pressure in a given vessel even when it leaked.

Lothar Meyer (*Ann. Chem. Pharm.* vol. clxv. p. 303) had already devised an apparatus of this kind, adapted chiefly to fractional distillation under reduced pressure. It consists essentially of two vertical tubes, A B and C D (fig. 1, Pl. XIII.), connected at the upper part by the lateral tubes E and F; at B an india-rubber tube connects A B with the tube K, which slides up and down the board to which the whole is fixed; at the top of A B is a tube H which is connected with an air-pump; the top of C D is provided with a cork, through which a narrow tube S S passes nearly to the bottom of C D; finally, the lateral tube *k* is connected with the apparatus X, in which a constant pressure is kept up. Sufficient mercury is poured into R to fill A B up to the lower end of H when R is about half full; C D is also filled with mercury, which is let out by the tap M until the column P S above the lower end of S S is equal to the difference between the required pressure and that of the atmosphere. If now the air-pump be set to work, a partial vacuum will be created in X and in the tubes A B and C D, the mercury will rise in A B until it touches the lower end of H; R is then placed in such a position that the vertical distance between the lower end of H and the surface of the mercury in R is equal to P S. It is now evident that when the desired pressure is reached the mercury will close up the orifice of H, thus stopping the withdrawal of air; while if the pressure is less than that re-

quired, more air will enter through the tube S S and bubble up through the mercury, and thus a more or less constant pressure will be maintained in X.

This apparatus suffers from two defects: first, the splashing of the mercury as it is sucked up H and then falls down again, together with the bubbling of air up C D, renders the pressure in X slightly variable; secondly, it is only adapted for pressures less than that of the atmosphere.

Meyer's instrument has very recently been modified by Dr. Otto Schumann and by W. Stadel and E. Hahn (*Ann. Chem. Pharm.* vol. cxcv. p. 218); the new form, although capable of regulating pressures above an atmosphere, apparently with tolerable accuracy, has a very limited range.

The apparatus for maintaining constant pressures, which I now wish to bring to the notice of the Society, consists of a manometer connected with which is an automatic arrangement for governing the supply of air. The manometer (fig. 2) consists of a tube A B having the form and dimensions shown in the figure; the lateral tube C is connected with the vessel X, in which a constant pressure is to be kept up; the tube D is connected with an air-pump or other contrivance for rarefying and compressing air. The upper end of the tube A B is closed by an india-rubber stopper, or, better, by a metal cap, through which the rod E_1 passes air-tight; this rod is tipped with platinum at its lower end. The lower end of A B is joined by means of india-rubber tube to the tube F, of which the upper part has the same diameter as that of A B; this tube is fitted with another cap and iron rod E_2 similar to the first; but the cap does not fit air-tight. The piece of wood which carries F moves along the scale S S in a groove in the board to which the whole is fixed. Lastly, the two rods E_1, E_2 are furnished with binding-screws for copper wire, while a third binding-screw K is connected with a small piece of steel tube interposed between the end of A B and the india-rubber tube. This third connexion with the mercury contained in the manometer may of course be made by means of a third (insulated) rod passing down to the bottom of the wider part of A B or F.

To fill and adjust the manometer, the tube F is raised to the same level as A, and mercury is poured in until it just reaches the wide cylindrical portions of A and F; the rods E_1, E_2 are

then moved until the point of E_1 just touches the surface of the mercury, while that of E_2 is a fraction of a millimetre above it.

Suppose now that F be lowered n millims. and the air-pump be set to work to pump air out at D; further, that K be connected with one pole P of a battery, and the rods E_1, E_2 with the other pole N; then it is evident that as long as the pressure in A, and consequently in X, is greater than $H-n$ (where H= the barometric pressure), the current will pass through the circuit P K E_2 N, whereas when the pressure is less than $H-n$ the current will pass along P K E_1 N. Similarly, if F be raised n millims. above A and the pump be made to compress air into A, as long as the pressure is less than $H+n$, the current will pass along P K E_1 N; but when the pressure becomes greater than $H+n$, the current will pass along P K E_2 N. In order to economize space, the tube A B is widened out at B; and when it is intended to obtain a pressure greater than H, the rod E_1 is replaced by a long one reaching down to B; this, of course, is equivalent to lowering A or raising F nearly the whole length of the scale S S. In the further description I shall only consider the case in which F is lower than A—that is, when a pressure less than H is required; the alteration needed for higher pressures will readily suggest itself.

The apparatus depicted in fig. 3 consists of a brass tap T and an electromagnetic clutch to work the tap automatically. The tap, which is shown in section in fig. 4, is placed between the tube D of the manometer (fig. 2) and the air-pump, L_1 being connected with D and L_2 with the pump. From the figure it is readily seen that when the tap is in the position drawn, and the lever fitted to the head of the key lies in the direction $a\,b$, air will be admitted into the manometer; if, on the other hand, the lever and key occupy the position $a'\,b'$, the pump will withdraw air from the manometer. The object of the clutch, therefore, is to place the tap in the first position when the pressure is too small, and in the second when it is too great. The lever $a\,b$ (fig. 3) terminates in two arcs which are grooved to hold a cord; these arcs are furnished with set screws S_1, S_2, by means of which the amount which the tap can open may be regulated. The two ends of the lever are connected by strings at S_1 S_2 to the

loose pulleys P_1 P_2 of the clutch. These pulleys are made of soft iron, and run on a spindle which revolves on the centres A_1, A_2. Revolving with the spindle and facing P_1 and P_2 are two small electromagnets M_1, M_2: one end of the coils of each of these magnets is soldered to the insulated ring I_2; the two other ends are soldered to I_1 and I_3 respectively. The binding-screws U_1, U_2, U_3 are connected electrically by means of springs with these rings; U_2 is further connected with the pole N of the battery, while from U_1 and U_3 wires run to E_2 and E_1 respectively. The disposition of the wires is shown in fig. 5: it is there evident that if the mercury in the manometer touches the rod E_1, M_2 will become magnetic; the loose pulley P_2 (fig. 3) will then tend to revolve with it, and the tap will take up the required position $a\,b$; while if the mercury touches E_2, M_1 will become magnetic, P_3 will revolve, and the tap will assume the position $a'\,b'$, in which communication is made with the air-pump. By this arrangement, therefore, the pressure in the manometer, and in whatever apparatus is connected with it, is caused to rise and fall within very small limits; with care these limits may be made to differ only about 0·25 millim.; and thus a practically constant pressure is attained.

The current required to work the magnets is no more than is furnished by a small Smee's cell; the magnets may be made to revolve with a small turbine; the air-pump may be replaced by a Bunsen water-pump; an air-pump, however, is the only convenient apparatus for *compressing* air.

It may be objected that the arrangement above described requires motive power, which is not always at hand in a laboratory. To meet this objection I endeavoured to construct a double valve to be moved to and fro by two stationary magnets; but I found that magnets of ordinary size were not powerful enough for the work, as the valve, to be of any use, must fit perfectly air-tight, even when subjected to very high pressures. I have not, however, given much attention to this point, an engine which I use for many other purposes being obviously the best source of the necessary power.

In fig. 6 is given a section of the steam- or vapour-bath which I employ for the comparison of thermometers and for the measurement of the expansion of liquids in dilatometers; it is also suitable for the direct comparison of thermometers with a

standard air-thermometer. The bath is made of brass, and consists merely of a boiler B surmounted by a double tube D,D similar to those first used by Rudberg and Regnault for the upper fixed point of thermometers; the vapour, after traversing this, passes into the U-shaped condenser C, from which the condensed liquid runs back into B down the tube E. The end H of this condenser is connected with the lateral tube C of the manometer by means of a very small lead pipe; and thus the liquid can be made to boil under any required pressure. The thermometers or other instruments are placed in the small tubes T T, which are filled with petroleum of high boiling-point. Besides water, the best liquids for generating the vapour appear to be carbon disulphide for low, and purified paraffin oil for high temperatures; the latter substance I have not hitherto used, as I have had no occasion to make observations at such temperatures. I have, however, made experiments with it in a smaller apparatus of similar form, and found that no variation of temperature took place.

The temperature obtained from these three liquids may be varied from 25° to 300° without unduly increasing the pressure.

If the temperature in the double tube D D be observed with a thermometer of which each degree is 5 millims. long, no variation can be detected, even with an extremely rigid cathetometer, provided, of course, that the barometric pressure does not alter; the variation due to this cause might, if necessary, be removed by making the cap of the tube F of the manometer fit air-tight. When great accuracy is necessary, it is not advisable to decrease the pressure below 100 millims.

In order to see whether the whole length of D D is at the same temperature, small oblique tubes similar to T were inserted, one at the top, the other at the bottom; a thermometer placed first in one and then in the other of these, gave exactly the same reading in both.

To sum up. First, with the above apparatus, viz. a manometer communicating by means of a specially constructed tap worked by a double electromagnetic clutch with a constantly working air-pump, any given pressure may be maintained for an indefinite period without varying more than 0·25 millim.; secondly, if this constant pressure be applied to a suitable

vapour-bath, any given temperature between 25° and 300° may be maintained absolutely constant as long as no chemical change occurs in the liquid whence the vapour is derived.

XI. *On the Fracture of Colloids.* By FREDERICK GUTHRIE*.

[Plates X. & XI.]

§ 1. A PROMINENT property with regard to solid colloids is that they have neither crystalline form nor planes of cleavage. When such a body is broken it offers the so-called conchoidal fracture. An agglomeration of crystals may present in mass the conchoidal fracture usually associated with colloids. This is the case with granite, and eminently so with basalt, all of whose constituents are crystalline. When the solid has resulted from the intersolution of two or more crystalloids it may, like glass, present the colloidal fracture in a most marked manner. And, indeed, even single crystals themselves are often subcolloidal in fracture; that is, conchoidal fracture accompanies the crystalline. This state is shown by the diamond, sugar-candy, quartz, &c.

I assume here that every cohesionally homogeneous mass of solid matter will break conchoidally when subjected to pressure sufficient to cause fracture.

Experiments.

§ 2. The cracking of a glass plate by pressure offers no special features of interest. A round plate placed on a thick soft cloth and pressed in the centre by a round cork cracks radially; the cracks are generally slightly curved. Fig. 1 shows two examples of fracture of crown glass by pressure in the centre. Similarly, if a round sheet of glass placed on a thick soft cloth be pressed down at its circumference by means of cardboard rings, the same class of crack is produced; for, indeed, the two conditions are essentially identical.

§ 3. The internal strain caused by difference of temperature causes fracture of great regularity and beauty. It rarely happens that a sheet of glass of any shape breaks into only two pieces when heated. If a circular piece of "crown"

* Read March 22, 1879.

Fig. 1.

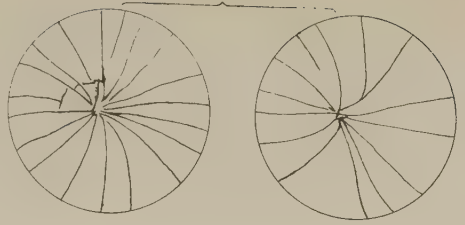


Fig. 2.

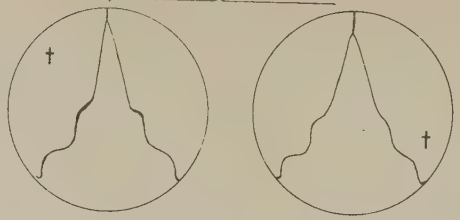


Fig. 3ª.

Fig. 3.

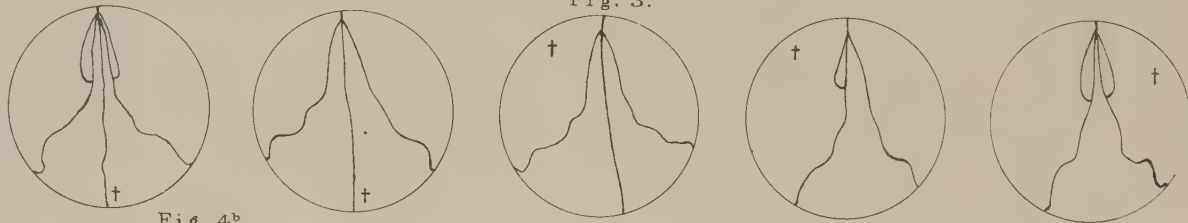


Fig. 4ª.

Fig. 4ᵇ.

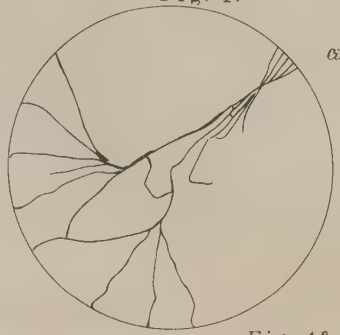


Fig. 4ᶜ.

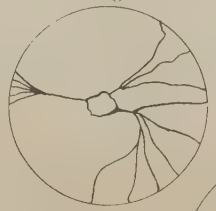


Fig. 5ª.

Fig. 5ᶜ.

Fig. 4ᵉ.

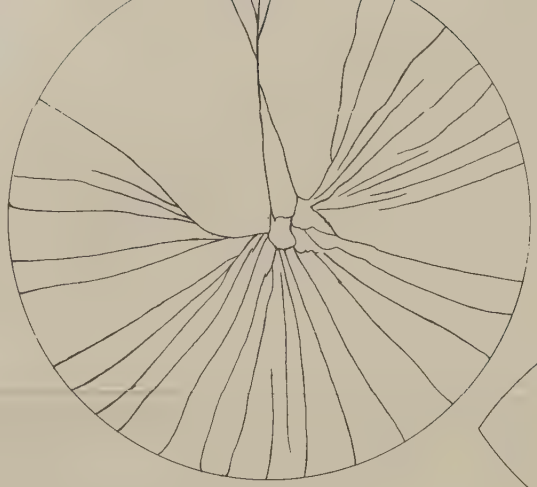


Fig. 5b.

Fig. 6.

Fig. 6.

Fig. 6.

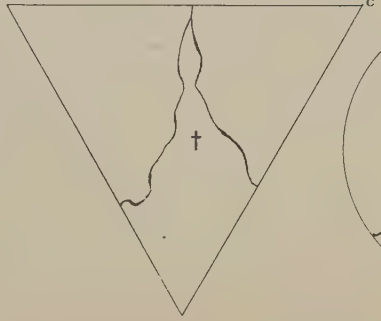


Fig. 6.

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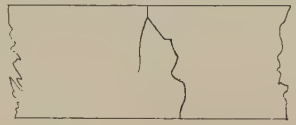


Fig. 6.

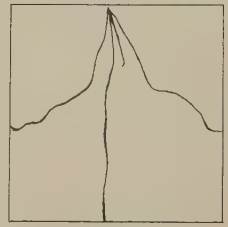


Fig. 7ª

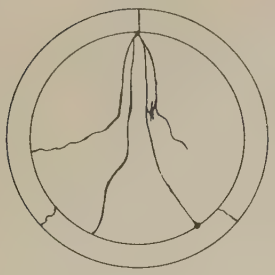


Fig. 7ᵇ

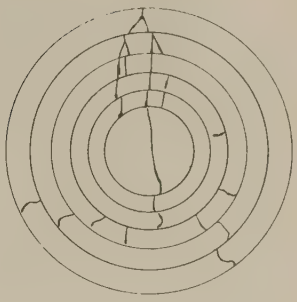


Fig. 8.

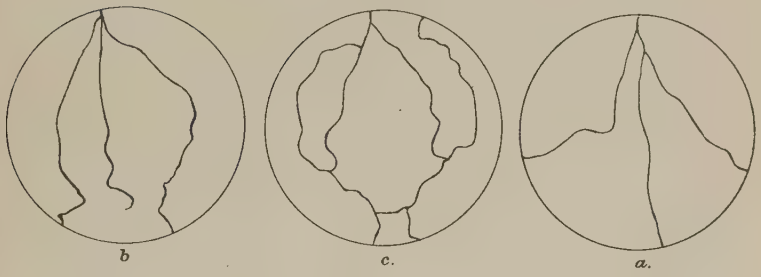
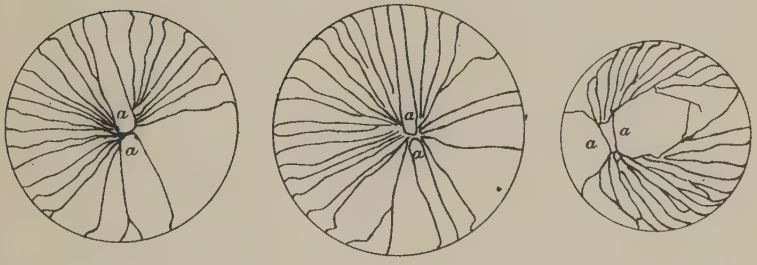


Fig. 9.



glass, about 3½ inches in diameter, be loosely balanced horizontally between the lips of a wooden clip and brought with its centre over an air-gas burner so that the top of the flame is about an inch below the glass, the latter almost invariably cracks at least into three pieces ; and when the pieces are three in number they as invariably have the form shown in fig. 2.

The remarkable symmetry of each of these, and their similarity to one another, show that the shape is not an accident of the glass. The constant features are (1) that the two main cracks join before reaching the circumference, (2) that there are in each crack, reckoned from this confluence, three concavities towards the centre of the circle, the first being nearly straight, (3) that there is a little kick given by the crack as it leaves.

Out of sixty-four specimens of fracture produced under these conditions ten showed this species of two-crack fracture. The shapes of the cracks are perfectly similar to those given ; and the symmetry is sometimes such that the side pieces may replace one another after inversion so perfectly that it is scarcely possible to tell that they are misplaced.

§ 4. The same method of heating may result in the production of a great variety of forms ; but they are all derived from the above type. In fig. 3 are shown a few of the more simple.

The three-crack figures (*a*, *a*) are about as frequently formed as the two-crack figures. I find eight of the three-crack out of sixty-four similarly treated specimens. Perfectly similar forms were got when the plate was laid on a retort-ring or supported on three corks and heated in the same manner. A special series of experiments was moreover made to see if the position of the clip had any influence upon the attitude of the crack. In figures 2 and 3 the mark † shows where the plate was held. As to the effect of the position in the original sheet of glass of the pieces experimented on, as determining the attitude of the axis of cracking, the following examination was made. Six pieces having been marked as they lay in the sheet, forming a radial band, were cut out and heated as above. The apex of crackage always appeared somewhere on the semicircle which was towards the centre of the sheet, but varied in this semicircle so considerably that

it is at present doubtful whether the position in the sheet influences the crack-axis. Fig. 4 shows the amount of variation; the point of the arrow represents the apex of the crack-curve. The lower figure represents the original sheet and the positions of the several pieces in it.

§ 5. If the sheet of glass be made very much larger, or the flame smaller and more pointed, another alteration of the crack-figure ensues. The apical point of the previous figures advances into the sheet; and this is followed by a fan-like cracking of the glass between the apex and the still nearest circumference. In fig. 4, *a* shows the cracking of a plate of crown glass, 9 inches in diameter, over an air-gas burner; *b* is a $5\frac{1}{2}$-inch-diameter plate similarly treated. In *c* we have a plate of crown glass, 3 inches in diameter, which was laid on a cloth and heated from above by a fine blowpipe-flame. If we conceive what was before called the apical point to reach the centre, the heat fracture would become approximately the central-pressure fracture, namely radial.

§ 6. A piece of plate-glass $\frac{1}{4}$ inch thick and a little over 7 inches in diameter, cracked when heated in the centre over an air-gas burner, as shown in fig. 5 *a*. A piece of "sheet-glass" (Chance's), 3 inches in diameter, cracked as shown in *b*. A slab of resin $\frac{1}{2}$ inch in thickness and $3\frac{1}{2}$ inches in diameter, heated in the centre by a jet of low-pressure steam, gave the fissures shown in *c*. Square porcelain tiles cracked nearly straight across in one crack.

§ 7. Pieces of crown glass of various shape were next examined, with the result which declares itself in fig. 6. The pieces were supported at the point marked *c*, and the flame applied below the point marked †.

The figure 6 shows that the same general type is preserved. It instructs us that the apical point seeks one of the nearest points of the circumference.

§ 8. Experiments were next made for the purpose of ascertaining under what circumstances, if at all, a crack could cross a crack. A circular plate of crown glass was cut by the diamond in concentric rings, and the crack was made to pass, by tapping, completely through the thickness of the glass, around the whole circumference. Such divided glass on being heated in the centre over an air-gas burner cracked according

to the same type as before. Sometimes the heat-crack would run across the diamond-crack, as though the latter had no existence. Sometimes the heat-crack would follow, and, as it were, adopt the diamond-crack, and then break off. In the latter of such cases the inner circle may be suffering a three-crack fracture, while the outer ring exhibits only a two-crack fracture on the converse. Fig. 7 (*a*) exhibits the former circumstance, fig. 7 (*b*) shows the influence of a greater number of concentric cracks.

§ 9. The heating of the central part of a circular plate should give the same crack-figure as the cooling of the circumference; and, as a matter of experiment, it is found that the figures are very similar. In order to cool the circumference of a heated circular plate with some approach to uniformity, an annular trough was constructed by cementing concentrically two glass cylindrical vessels of the same depth but different diameters, one inside the other, and over-filling with mercury, so that the convex surface of the metal projected. The glass was held in a wooden clip widely stretched, so that the axis of the clip being vertical the plate was horizontal. Held, when uniformly hot, immediately above the mercury, it was let drop and pressed down in the middle by a piece of wood. The fracture is in this case instructive; for while in fig. 8 (*a*) the old type got by heating the centre is resumed in *b* and *c*, the fracture is either influenced or even accompanied by the circular fracture along or near the line of greatest temperature-difference.

§ 10. It is clear that heating in the central regions should produce a similar fracture to that brought about by cooling around the circumference, and cooling at the centre a similar fracture to that caused by heating the circumference.

On heating a circular plate at the circumference by means of a "rose" air-gas burner, it breaks with far greater violence than when fracture is produced by central heating. The parts are scattered at least three times as far in the former as in the latter case. The form of the fracture is essentially radial; but the fragments, even when the primitive type is widely departed from, present wonderful symmetry. A noticeable point in this fashion of fracture is the invariable appearance of two pieces on opposite sides of the centre whose

form is approximately rectangular ; that is, their sides are half-cords instead of radii. This form suggests that there are two chief centres of maximum fracture, and that the bounding radii of the two systems are parallel. In fig. 9 the pieces marked *a* represent these singular pieces. Out of seven plates which have been broken in this way, there is not one in which this feature is absent.

§ 11. As to cooling a hot plate in the centre, I find such extreme difficulty in reproducing the inverse conditions of heating a hot plate at the circumference that I have rarely succeeded in reproducing the same type of fracture.

Also it is seldom the case that a sheet of glass cracks during heating at its edge. More frequently a sheet of glass which has been heated at its edge cracks when cooling. The crack then appears to follow that isothermal line along which there is the greatest difference of temperature at right angles to the line.

Remarks.

§ 12. About the fracture by mechanical strain it may appear to satisfy many that the lines of fracture are perpendicular to that resultant of the pressure which lies in the plane fractured. A tear in a sheet of paper is at right angles to the two opposing pressures, or rather to their resultants at the point yielding.

What is a crack? Which are its beginning and end? In only one of the above-cited experiments can the growth of a crack be followed. In § 11 when a plate heated at the edge has refused to crack while being heated but cracks on cooling, the crack is seen to extend from the edge of the plate inwards, following, generally speaking, a semicircular path, but sometimes curiously modified towards the centre of the curve.

A crack is neither a line of least cohesion nor a line of greatest strain. Nor is it a line where $\frac{c}{s}$ has a series of minimum values. The more perfectly elastic a medium is, the more fully does the crack resemble a flash of lightning or wisely laid railway-line, and the more it departs from the river-course or the descent of a globule of mercury down an inclined but undulating surface. Its path is the curve whose

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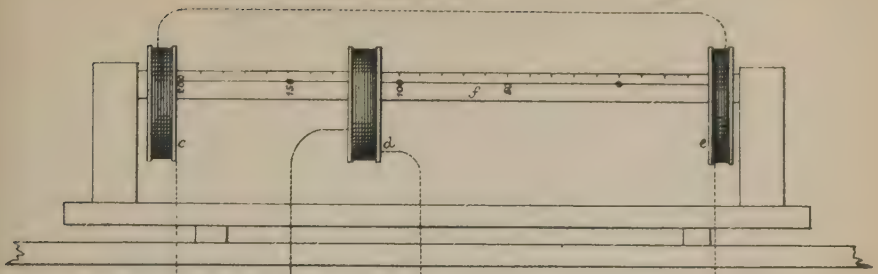

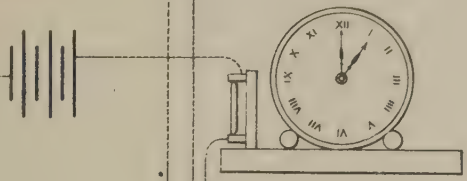

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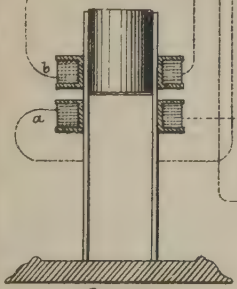

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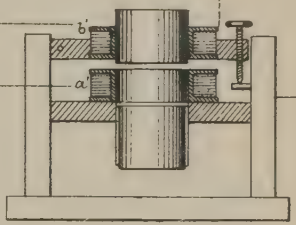

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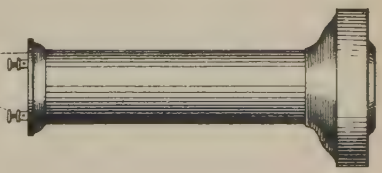
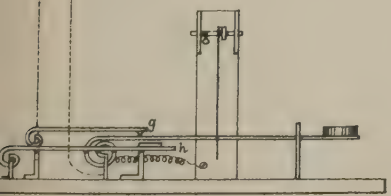




SWITCH-KEY

course is determined by the integnal of $\frac{c}{s}$ being a minimum.

The sudden splitting through of the solid æther by the electric discharge furnishes us with figures by no means remotely resembling those of the fracture of glass. Even or rather especially the forms of fig. 4 remind us of this.

As to the typical form in fig. 2, it has been suggested by my brother, Mr. Charles Guthrie, that this form is a compromise between the circular line of fracture along some isothermal line where the difference of temperature is greatest, or rather the difference of expansion is greatest, with the three lines of relief which would be radii at angles of 120° with one another.

This is a very suggestive hint ; but, for reasons sufficiently apparent from the foregoing, it is insufficient.

XII. *Induction-balance and Experimental Researches therewith.* *By* Prof. D. E. Hughes.

[Plate XII.]

Immediately upon the announcement of Arago's discovery of the influence of rotating plates of metal upon a magnetic needle (1824), and Faraday's important discovery of voltaic and magneto-induction (1831), it became evident that the induced currents circulating in a metallic mass might be so acted upon either by voltaic or induced currents circulating in a metallic mass as to bring some new light to bear on the molecular construction of metallic bodies.

The question was particularly studied by Babbage, Sir John Herschell, and M. Dove, who constructed an induction-balance, wherein two separate induction-coils, each having its primary and secondary coils, were joined together in such a manner that the induced current in one coil was made to neutralize the induced current in the opposite coil, thus forming an induction-balance, to which he gave the name of "differential inductors." In those days physicists did not possess the exquisitely sensitive galvanometers and other means of research that we possess today; but sufficiently important results were obtained to prove that a vast field of research would be opened

if a perfect induction-balance could be found, together with a means of correctly estimating the results obtained. In experimenting with the microphone I had ample occasion to appreciate the exquisite sensitiveness of the telephone to minute induced currents. This led me to study the question of induction by aid of the telephone and microphone: the results of those researches have already been published. Continuing this line of inquiry, I thought I might again attempt to investigate the molecular construction of metals and alloys; and with this object I have obtained, after numerous comparative failures, a perfect induction-balance which is not only exquisitely sensitive and exact, but allows us to obtain direct comparative measures of the force or disturbances produced by the introduction of any metal or conductor.

The instrument now submitted to the Physical Society consists, 1st, of the new induction-currents balance; 2nd, a microphone, with a clock as a source of sound; 3rd, electric sonometer, or absolute sound-measurer, a late invention of my own; 4th, a receiving telephone and three elements of Daniell's battery. In order to have a perfect induction-currents balance suitable for physical research, all its coils as well as the size and amount of wire should be equal. The primary coils a, a', and the secondary coils b, b', should be separated and not superposed. The exterior diameter of the coils is $5\frac{1}{2}$ centims., having an interior vacant circular space of 3 centims.; the depth of this flat coil or spool is 10 millims. Upon this box-wood spool are wound 100 metres of No. 32 silk-covered copper wire. I use four of such coils, formed into two pairs, the secondary coil being fixed permanently, or by means of an adjustable slide, at a distance of 5 millims. from its primary; on the second similar pair there is a fine micrometer-screw, allowing me to adjust the balance to the degree of perfection required. These two pairs of coils should be placed at a distance not less than one metre from each other, so that no disturbing cause may exist from their proximity. The two primary coils are joined in series to the battery, the circuit also passing through the microphone. In place of the telephone I have sometimes used a magnetic pendulum, the swing or the arc described indicating and measuring the forces. I am at present engaged upon a very sensitive volta-

meter, which shall indicate and measure the force of rigid induced currents. The telephone, however, is well adapted as an indicator, but not as a measure of the forces brought into action. For this reason I have joined to this instrument an instrument to which I have given the name of electric sonometer (Pl. V.) This consists of three coils *c*, *d*, *e*, similar to those already described, two of which are placed horizontally at a fixed distance of about 40 centims. apart; and the communication with battery is so arranged that there are similar but opposing poles in each coil; between these two there is a coil, *d*, which can be moved on a marked sliding scale, *f*, divided into millimetres, in a line with these two opposing primary coils*. This central coil is the secondary one, and connected by means of a switching key with the telephone in place of the induction-balances. If this secondary coil is near either primary coil, we hear loud tones, due to its proximity. The same effect takes place if the secondary coil is near the opposing coil, except that the induced current is now in a contrary direction, as a similar pole of the primary acts now on the opposite side of the induction-coil. The consequence is, that as we withdraw it from one coil and approach the other, we must pass a line of absolute zero, where no current whatever can be induced, owing to the absolute equal forces acting equally on both sides of the induction-coil. This point is in the exact centre between the two coils.

We thus possess a sonometer having an absolute zero of sound: each degree that it is moved is accompanied by its relative degrees of increase; and this measure may be expressed in the degrees of the millimetres passed through, or by the square of the distances in accordance with the curve of electro-magnetic action. If we place in the coils of the induction-balance a piece of metal (say copper, bismuth, or iron), we at once produce a disturbance of the balance, and it will give out sounds more or less intense on the telephone according to the mass, or if of similar sizes, according to the molecular structure of the metal. The volume and intensity of sound is invariably the same for a similar metal. If by means of the switching key the telephone is instantly transferred to the

* If the coils *c* and *e* are of unequal size, the zero of the scale will occupy a position similar to that shown on the Plate.

sonometer, and if its coil be at zero, we hear sounds when the key is up, or in connexion with the wire *g*, which leads to the induction-balance, and no sounds, or silence, when the key is down or in contact with the wire *h*, in connexion with the sonometer. If the sonometer-coil were moved through several degrees, or through more than the required amount, we should find that the sounds increase when the key is depressed; but when the coil is moved to a degree where there is absolute equality, if key is up or down, then the degree on scale should give the true value of the disturbance produced in the induction-balance; and this is so exact that if we put, say, a silver coin whose value is 115°, no other degree will produce equality. Once knowing, therefore, the value of any metal or alloy, it is not necessary to know in advance what the metal is; for if its equality is 115°, it is silver coin; if 52, iron; if 40, lead; if 10, bismuth; and as there is a very wide limit between each metal, the reading of the value of each is very rapid, a few seconds sufficing to give the exact sound-value of any metal or alloy.

The respective values of the different metals may, as I have already pointed out, be indicated by introducing the sonometer into circuit. I find, however, that it is difficult to estimate fractional differences when the sounds to be compared are loud. I therefore prefer to *balance* the metal under examination by means of a similar mass placed in the opposing coil, reading on the sonometer the differences of sound, which are then slight. Experience has shown that the most accurate results are obtained when the sonometer is replaced by a graduated strip of zinc about 23 millims. wide, 200 millims. long, and tapering from a thickness of 4 millims. at one end to a fine edge at the other, and superposed in a horizontal plane over the opposing coil *b'*, the metal to be tested being in a plane midway between *a* and *b* on the left of the plate.

The delicacy of the readings may of course be greatly increased by diminishing the angle between the two faces of the strip; but there are many points connected with its use which would be too long to describe in this paper.

As a rule three Daniell elements will be found quite sufficient; and even this weak force is so exquisitely sensitive that it will find out the smallest fraction of difference in weight or

structure of metals. Thus two silver coins such as a shilling, both quite new, and both apparently of the same weight, will be found to possess a difference of weight which the instrument at once indicates.

The following experiments will show its exceeding sensitiveness and its wide field of usefulness as an instrument of research.

I. If we introduce into one pair of the induction-coils any conducting body, such as silver, copper, iron, &c., there are set up in these bodies electric currents which react both upon the primary and secondary coils, producing extra currents whose force will be proportional to the mass and to its specific conducting-power. A miligramme of copper or a fine iron wire, finer than the human hair, can be loudly heard and appreciated by direct measurement, and its exact value ascertained. We can thus weigh to an almost infinitesimal degree the mass of the metal under examination: for instance, if we take two English shilling pieces fresh from the Mint, and if they are absolutely identical in form, weight, and material, they will be completely balanced by placing one each in the two separate coils, provided that for these experiments there is an adjustable resting-place in each pair of coils, so that each coin may lie exactly in the centre of the vacant space between the primary and secondary coils. If, however, these shillings are in the slightest degree worn, or have a different temperature, we at once perceive this difference, and, if desired, measure it by the sonometer, or by lifting the supposed heaviest coin at a slight distance from the fixed centre line: the amount of degrees that the heaviest coin is withdrawn will show its relative mass or weight as compared with the lightest. I have thus been able to appreciate the difference caused by simply rubbing the shilling between the fingers, or the difference of temperature by simply breathing near the coils; and in order to reduce this sensibility within reasonable limits, I have only used in the following experiments 100 metres of copper wire to each coil, and 3 cells of battery.

II. The comparative disturbing value of disks of different metals, all of the same size and form of an English shilling, and measured in millimetre-degrees, by the sonometer, is the following:—

Silver (chemically pure)	125
Gold „ „	117
Silver (coin)	115
Aluminium	112
Copper	100
Zinc	80
Bronze	76
Tin	74
Iron (ordinary)	52
German silver	50
Iron (chemically pure)	45
Copper (antimony alloy)	40
Lead	38
Antimony	35
Mercury	30
Sulphur (iron alloy)	20
Bismuth	10
Zinc (antimony alloy)	6
Spongy gold (pure)	3
Carbon (gas)	2

III. It will be seen from the above that the instrument gives very different values for different metals or alloys ; consequently we cannot obtain a balance by employing two disks of different metals, and the instrument is so sensitive to any variation in mass or matter that it instantly detects the difference by clear loud tones on the telephone. If I place two gold sovereigns of equal weight and value, one in each coil, there is complete silence, indicating identity or equality between them ; but if one of them is a false sovereign, or even gold of a different alloy, the fact is instantly detected by the electrical balance being disturbed. The instrument thus becomes a rapid and perfect coin-detector, and can test any alloy, giving instantly its electrical value. The exceeding sensitiveness of this electrical test I shall demonstrate by experiment, now. Again, as regards coins, it resolves an almost magical problem. Thus, if a person puts one or several coins into one pair of coils, the amount or nominal value being unknown to myself, I have only to introduce into the opposite coils different coins successively, as I should weights in a

scale, and when perfect balance is announced by the silence, the amount in one box will not only be the same nominal value but of the same kind of coin.

IV. We find, by direct experiment with this instrument, that the preceding results are due to electric currents induced by the primary coil, and that it is by the reaction of these that the balance is destroyed; for if we take an insulated spiral disk or helix of copper wire with its terminal wires open, there is no disturbance of the balance whatever, notwithstanding that we have introduced a comparatively large amount of copper wire; but on closing the circuit the balance is at once very powerfully disturbed.

If the spiral is a flat one, resembling a disk of metal, and circuit closed, we find that loud tones result when the spiral is placed flat, or when its wire is parallel to those on the coils; but if it is held at right angles to these wires, no sound whatever is heard, and the balance remains perfect. The same thing occurs with disks of all non-magnetic metals, and a disk of metal placed perpendicular to the coils exerts no influence whatever. The contrary result takes place with a spiral of iron wire or disk of iron: the induced current circulating in the spiral is at its maximum when the spiral lies flat or parallel with the coils, giving no induced current whatever when at right angles; but the disturbances of the induction-balance are more than fourfold when perpendicular to the wires of the coils than when parallel with the same. This result is simply due to the property of magnetic bodies, of conduction of magnetism.

V. If we introduce a disk of metal gradually into the coils, we find that its power increases as the square of the distance, until it arrives at its maximum exactly in the centre of the vacant space between each pair of coils, diminishing rapidly in the same ratio if the disk be moved towards primary or secondary. Thus, in the interior of the coils there is but a single line of maximum force; but at the exterior we have on each coil two maxima and one minimum, the first and most powerful maximum being in the centre of vacant space between each pair of coils, coinciding with the maximum lines of force of the centre of the coils; the minimum lines of force are exactly in the exterior centre of each coil, again

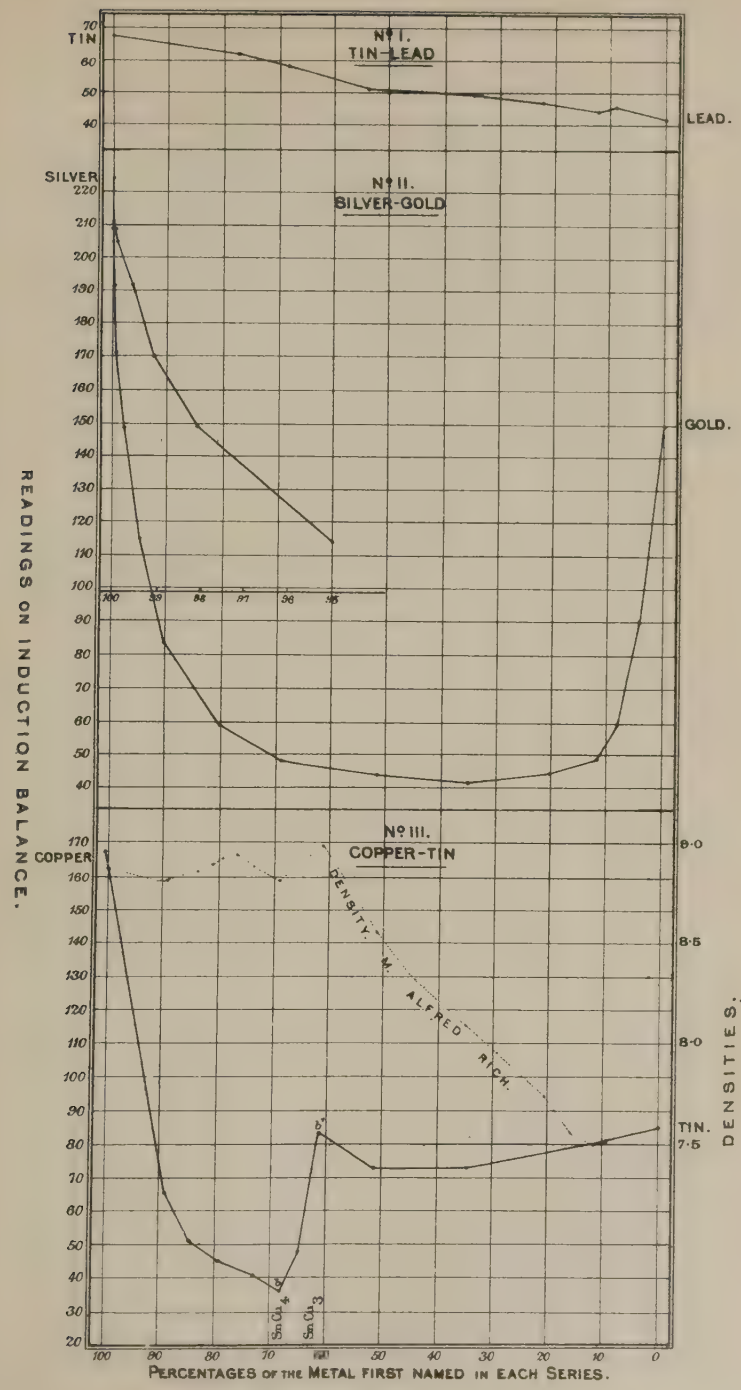
rapidly rising to a maximum near the exterior edge of coils, and gradually diminishing in power from this point.

If we place exteriorly a bar of metal in the centre line of vacant space, we find that it has here its maximum disturbing power, giving out loud sounds.

If we now move this bar until it rests on the centre of either coil, we find that at this point the bar has no disturbing effect whatever, and although the coils at its maximum line of force are sensitive to the finest iron wire, a very large mass of iron or a rod of 1 centimetre diameter has no disturbing effect whatever. Its paralyzing effects are so remarkable, that if we place a flat piece of iron or other metal across the maximum line of force, where loud sounds are given out, the instant that this flat piece is moved, so that one or both edges touch or end in the minimum line of force, it becomes instantly neutralized, giving out no sound whatever, notwithstanding that a large mass of metal lies in the maximum line of force. We will now demonstrate by experiment the exceeding sensitiveness of the induction-balance to the smallest piece of metal, if it is in the maximum line of force, and no part of it touching the minimum; and also that by allowing either or both ends of this or a very large piece of metal to cut or end at the minimum lines of force, complete paralysis and consequent silence are produced*.

VI. There is a marked difference of the rapidity of action between all metals, silver having an intense rapidity of action. The induced currents from hard steel or from iron strongly magnetized are much more rapid than those from pure soft iron; the tones are at once recognized, the iron giving out a dull, heavy smothered tone, whilst hard steel has tones exceedingly sharp. If we desire to balance iron, we can only balance it by a solid mass equal to the iron to be balanced. No amount of fine wires of iron can balance this mass, as the time of discharge of these wires is much quicker than that of a larger mass of iron. Hard steel, however, can be easily balanced not only by steel but by fine iron wires, and the degree of the fineness of these wires required to produce a balance gives a very fair estimate of the proportionate time

* This was fully demonstrated to the audience by numerous experiments.


No I.
TIN-LEAD
TIN
LEAD.
No II.
SILVER-GOLD
SILVER
GOLD.
100 39 86 171 36 95
No III.
COPPER-TIN
COPPER
TIN.
DENSITY. M. ALFRED RICH.
8·0
8·5
8·0
7·5
DENSITIES.
READINGS ON INDUCTION BALANCE.
Sn Cu 4
Sn Cu 3
100 90 80 70 60 50 40 30 20 10 0
PERCENTAGES OF THE METAL FIRST NAMED IN EACH SERIES.

of discharge. The rapidity of discharge has no direct relation with its electrical conductivity; for copper is much slower than zinc, and they are both superior to iron.

We find that the induction-balance is exceedingly sensitive to all molecular changes which take place in all metals subjected to any of the imponderable forces. Thus we have already by its aid studied the effects on metals of heat, magnetism, electricity, &c., and of mechanical changes such as strain, torsion, and pressure; and I propose in some future paper to describe the remarkable results already obtained, and to demonstrate by experiment today to the Physical Society the results of these forces upon the induction-balance.

XIII. *Note on the Examination of certain Alloys by the Aid of the Induction-balance.* *By* W. Chandler Roberts, *F.R.S., Chemist of the Mint.*

[Plate XIII.]

Some weeks since, Prof. Hughes showed me that equal volumes of various metals give widely different indications with the induction-balance. It appeared probable that a careful examination of a definite series of alloys would prove to be of interest; and as Prof. Hughes at once gave me the most generous assistance, teaching me the manipulation and controlling the results, I am able to submit the following observations to the Society.

The relative values of different metals as indicated by the induction-balance were given by Prof. Hughes in a paper read before the Royal Society on the 15th of May last. They do not accord with the values usually accepted as representing the relative conductivity of the respective metals; and this being the case, it became important to ascertain what relation the indications given by alloys, when under the influence of the induced current, bear to their electric conductivities, which afforded Matthiessen a basis for dividing them into groups*.

A series typical of each group was therefore taken; the constituent metals were melted together in the requisite proportions; and the thoroughly mixed alloys were carefully rolled

* British Association Report, 1863, p. 37.

to a uniform thickness, usually 1·3 millim. Disks 24 millims. in diameter were then cut with the same punch; and these disks were placed in succession on one side of the balance, so that their bases lay exactly on a line midway between the primary and secondary coils, this having been found to be the plane of maximum force. The respective values of the alloys were ascertained either by introducing the sonometer into circuit, or by superposing a graduated wedge-shaped scale of zinc over the opposing coil of the balance, as has already been explained by Prof. Hughes.

The alloys of Lead and Tin were selected as an example of Matthiessen's first group. The results are recorded in the following Table, and are graphically indicated in the curve No. 1, Plate VI. The readings are those of the zinc scale*.

The Gold-Silver alloys, representing the second group, presented no difficulty of manipulation; and the observations were made on disks 1·3 millim. thick and 24 millims. in diameter. The results are given in the Table and in curve No. II.

The alloys of Tin and Copper, taken as representative of the third group, are peculiar. Their tints and fractures are widely different; and the series is interesting as having various industrial applications. As many of them are too brittle to roll, a block of each alloy 18 millims. square by 7 millims. thick was formed with the file. The results are given in the Table (p. 91) and on the curve No. III.

If the curves for Lead-Tin and Gold-Silver are compared with those given by Matthiessen † for the same alloys, their similarity will at once be evident. On the other hand, the induction-balance curve of the Tin-Copper series, while bearing some general resemblance to Matthiessen's curve of conductivity, differs essentially from it in certain parts. Matthiessen's curve falls rapidly from 93 (the conductivity of pure copper) to 9 (that of the alloy containing 85 *volumes* per cent. of copper). It then passes horizontally in a line which is approximately straight to 13, the conductivity of Tin.

* The use of a scale of greater accuracy than the one employed may slightly alter some of the figures, but it can hardly change the general nature of the curves.

† *Op. cit.* p. 46, and Watts's Dictionary of Chemistry, vol. iii. p. 943.

	No.	Percentage composition.	Approximate formula.	Readings on Induction-balance.
Tin-Lead.	1.	100 (pure tin).		67·5
	2.	77·37	$Sn_6 Pb$	62·0
	3.	69·60	$Sn_4 Pb$	59·0
	4.	53·20	$Sn_2 Pb$	52·5
	5.	36·30	$Sn\ Pb$	51·0
	6.	22·30	$Sn\ Pb_2$	47·0
	7.	12·50	$Sn\ Pb_4$	45·0
	8.	8·60	$Sn\ Pb_6$	46·0
	9.	0 (pure lead).		43·0
Silver-Gold.	1.	100 (pure silver).		225
	2.	99·97		209
	3.	99·90		205
	4.	99·50		192
	5.	99·10		170
	6.	98·02		148
	7.	94·93		115
	8.	90·00		84
	9.	81·40	$Ag_8 Au$	60
	10.	68·70	$Ag_4 Au$	48
	11.	52·30	$Ag_2 Au$	44·5
	12.	35·50	$Ag\ Au$	42·8
	13.	21·50	$Ag\ Au_2$	44
	14.	12·00	$Ag\ Au_4$	49
	15.	8·30	$Ag\ Au_6$	60
	16.	4·1		90
	17.	0 (pure gold).		150
Copper-Tin.	1.	100 (pure copper).		167
	2.	89·00	$Sn\ Cu_{15}$	65
	3.	84·33	$Sn\ Cu_{10}$	51
	4.	79·02	$Sn\ Cu_7$	45
	5.	72·91	$Sn\ Cu_5$	40
	6.	68·28	$Sn\ Cu_4$	37
	7.	65·00		49
	8.	61·79	$Sn\ Cu_3$	83
	9.	51·84	$Sn\ Cu_2$	73
	10.	34·99	$Sn\ Cu$	73
	11.	9·73	$Sn_5 Cu$	82
	12.	0 (pure tin).		85

Note.—The Alloys were not annealed; and the temperature was about 15° C.

Some light would appear to be thrown on the difference between the two curves by the work of M. Alfred Rich* on the density of alloys of copper and tin. He showed that copper and tin contract in alloying, the contraction being regular from pure tin up to the alloy containing 38 per cent. of tin, the density of which is higher than that of pure copper.

* *Ann. de Chimie et de Phys.* tome xxx. 1873.

M. Rich's experiments were conducted on alloys both in the form of powder and ingots; the latter have alone been given in the curve marked with his name on the Plate; and the relation between the two curves, especially at the points a, a', b, b', is too evident to need comment.

It may ultimately prove that if the alloys were rolled or compressed the curve would be modified; and, on the other hand, further experiments on the conductivity of the alloys may reveal points of identity between the conductivity and induction-balance curves; the part where the former from being vertical becomes horizontal would be especially worth examination. It may be well to point out that the alloys $SnCu_3$ and $SnCu_4$, which occupy critical positions on the induction curve, have been shown by M. Rich to be singularly free from the disturbing influence of liquation.

The work would appear to be interesting as showing that the induction-balance may afford a simple means for detecting variations in the molecular structure of alloys and for indicating allotropy in metals with greater accuracy than has hitherto been possible.

Practical application.—The possibility of ascertaining the standard fineness of alloys by the aid of electricity long ago occupied the attention of physicists. In 1823 M. Becquerel* suggested that trustworthy indications might be afforded by the electromotive force developed when the alloy is placed in an exciting fluid, together with an alloy of known composition. The subject was partially investigated by Œrsted in 1828†; and as its practical importance was further indicated by Gay-Lussac in 1830‡, I made a series of experiments in order to ascertain how far the more delicate appliances in use at the present day could be made available. The results, however, were not entirely satisfactory.

Prof. Hughes's Induction-balance rendered it possible to resume the research on a new basis. It is only necessary to glance at such a curve as that of the Gold-Silver series No. II. to be satisfied of the probability that certain parts of it, at least,

* *Ann. de Chimie et de Phys.* t. xxiv. p. 343 (1823).

† *Ibid.* t. xxxix. 1828, p. 274.

‡ *Instruction sur l'Essai des Matières d'Argent par la Voie Humide.* Paris, 1830.

would indicate minute differences of standard. I would therefore direct special attention to the series of alloys which lie between pure silver and silver alloyed with 5 per cent. of gold. These are shown in a separate curve, where the scale of percentages is more extended. Such alloys as Nos. 2 to 6 are known to refiners as doré; and No. 2 contains less than 2 grains of gold to the pound troy, a quantity which could not be extracted with profit by the ordinary operation of "parting." Small as the amount of precious metal is, its presence is clearly indicated on the induction curve, as are also the larger amounts of gold contained in Nos. 3–5.

Experiments are in progress on other series which promise to afford trustworthy indications; but of course the establishment of a method of verifying the composition of alloys of the precious metals must in part depend on the degree to which the presence of traces of foreign metals influence the accuracy of the results.

My object in these notes is not to insist on any particular application, but to bear testimony from a metallurgical point of view to the delicacy and simplicity of the instrument which Prof. Hughes has placed at our disposal; and I would offer him my sincere thanks for the liberal aid he has so readily given me.

XIV. *On* Professors Ayrton *and* Perry's *new Theory of the Earth's Magnetism, with a Note on a new Theory of the Aurora. By* H. A. Rowland, *Professor of Physics in the Johns Hopkins University**.

Some years ago, while in Berlin, I proved by direct experiment that electric convection produced magnetic action; and I then suggested to Professor Helmholtz that a theory of the earth's magnetism might be based upon the experiment. But upon calculating the potential of the earth required to produce the effect, I found that it was entirely too great to exist without producing violent perturbations in the planetary movements, and other violent actions.

I have lately read Professor Ayrton and Perry's publication

* Read June 29th.

of the same theory; and as they seem to have arrived at a result for the potential much less than I did, I have thought it worth while to publish my reasons for the rejection of the theory.

The first objection to the theory that struck me was, that not only the relative motion but also the absolute motion through space of the earth around the sun might also produce action. And to this end I instituted an experiment as soon as I came home from Berlin.

I made a condenser of two parallel plates with a magnetic needle enclosed in a minute metal box between them; for I reasoned that, when the plates were charged and were moved forward by the motion of the earth around the sun, they would then act in opposite directions on the enclosed needle, and so cause a deflection when the electrification of the condenser was reversed. On trying the experiment in the most careful manner, there was not the slightest trace of action after all sources of error had been eliminated.

But the experiment did not satisfy me, as I saw there was some electricity on the metal case surrounding the needle. And so I attacked the problem analytically, and arrived at the curious result that if an electrified system moves forward without rotation through space, the magnetic force at any point is dependent on the electrical force at that same point—or, in other words, that all the equipotential surfaces have the same magnetic action. Hence, when we shield a needle from electrostatic action, we also shield it from magnetic action.

This theorem only applies to irrotational motion, and assumes that the elementary law for the magnetic action of electric convection is the same as the *most simple* elementary law for closed circuits. Hence we see that, provided the earth were uniformly electrified on the exterior of the atmosphere, there would be no magnetic action on the earth's surface due to mere motion of translation through space.

In calculating the magnetic action due to the rotation, I have taken the most favourable case, and so have assumed the earth to be a sphere of magnetic material of great permeability, μ. It does not seem probable that it would make much difference whether the inside sphere rotated or was stationary; or at least the magnetic action would be greatest in the latter case;

and hence by considering it stationary we should get the superior limit to the amount of magnetism.

Let a be the radius of the sphere moving with angular velocity w, and let σ be its surface-density in electrostatic measure, and n the ratio of the electromagnetic to the electrostatic unit of electricity. Then the current-function will be

$$\phi = \frac{\sigma}{n} wa^2 \int \sin\theta \, d\theta = -\frac{\sigma}{n} wa^2 \cos\theta.$$

Hence (Maxwell's 'Treatise,' § 672) the magnetic potential inside the sphere is

$$\Omega = -\frac{8\pi}{3} \frac{\sigma}{n} war \cos\theta,$$

and outside the sphere

$$\Omega' = \frac{4}{3} \pi \frac{\sigma}{n} wa^4 \frac{\cos\theta}{r^2}.$$

The magnetic force in the interior of the sphere is thus

$$\mathrm{F} = \frac{8}{3} \pi \frac{\sigma}{n} wa,$$

or the field is uniform. If the electric potential of the sphere on the electrostatic system is V, we may write

$$\mathrm{F} = \frac{2}{3} \frac{w}{n} \mathrm{V},$$

which is independent of the dimensions of the sphere.

In this uniform field in the interior of the sphere, let a smaller sphere of radius a' be situated; the potential of its induced magnetization will be

$$\Omega_1 = \frac{\mu - 1}{\mu + 2} a'^3 \mathrm{F} \frac{\cos\theta}{r^2}.$$

Hence the expression for the potential for the space between the two spheres will be

$$\mathrm{U} = \frac{2}{3} \frac{w}{n} \mathrm{V} \left\{ -r\cos\theta + \frac{\mu - 1}{\mu + 2} a'^3 \frac{\cos\theta}{r^2} \right\};$$

and outside the electrified sphere it will be

$$\mathrm{U}' = \frac{w}{n} \mathrm{V} \left(a^3 + 2a'^3 \frac{\mu - 1}{\mu + 2} \right) \frac{\cos\theta}{r^2}.$$

Let us now take the most favourable case for the production of magnetism that we can conceive, making $a' = a$ and $\mu = \infty$;

we then have

$$U' = \frac{w}{n} V a^3 \frac{\cos\theta}{r^2},$$

which is the potential of an elementary magnet of magnetic moment

$$\frac{w}{n} V a^3.$$

But Gauss* has estimated the magnetic moment of the earth to be

$$3{\cdot}3092\, a^3$$

on the millimetre mg. second system. Hence we have

$$V = 3{\cdot}3092 \frac{n}{w}$$

for the potential in electrostatic units on the mm. mg. second system. In electromagnetic units it is thus

$$V_1 = 3{\cdot}3092 \frac{n^2}{w};$$

and hence in volts it is this quantity divided by 10^{11}.

As the earth makes one revolution in $23^h\ 56'\ 4''$, or in 86164 seconds, we have

$$w = \frac{2\pi}{86164};$$

and

$$n = 299{,}000{,}000{,}000\dagger \text{ millims. per second.}$$

Hence the earth must be electrified to a potential of about

$$41 \times 10^{15} \text{ volts}\ddagger$$

in order, under the most favourable circumstances, to account

* Taylor's Scient. Mem. vol. ii. p. 225.

† From a preliminary calculation of a new determination made with the greatest care, and having a probable error of 1 in 1300.

‡ That this is not too great may be estimated from my Berlin experiment, where a disk moving 5,000,000 times as fast as the earth with a potential of 10,000 volts, produced a magnetic force of $\frac{1}{50000}$ of the earth's magnetism,

$$5{,}000{,}000 \times 10{,}000 \times 50{,}000 = 2{,}500{,}000{,}000{,}000{,}000,$$

which is of the same order of magnitude as the quantity calculated namely 61×10^{15}. It can be seen that this reasoning is correct, because the formulæ show that two spheres of unequal size, rotating with equal angular velocity and charged to the same potential, produce the same magnetic force at similar points in the two systems.

for the earth's magnetism. This would be sufficient to produce a spark in atmospheric air of ordinary density of about

6,000,000 miles!

Professors Ayrton and Perry have only found the potential 10^8 volts, or 400,000,000 times less than I find it.

It was this large quantity which caused me to reject the theory; for I saw what an immense effect it would have in planetary perturbations; and I even imagined to myself the atmosphere flying away, and the lighter bodies on the earth carried away into space by the repulsion. And, doubtless, had not Professors Ayrton and Perry made some mistake in their calculation by which the force was diminished 16×10^{16} times, they would have feared like results.

For according to Thomson's formula, the force would be equal to a pressure outwards of

$$r = \frac{V^2}{8\pi a^2},$$

which amounts to no less than

1,800,000 grms.

per square centimetre! or 10,000 kil. per square inch! Such an electrostatic force as this would undoubtedly tear the earth to pieces, and distribute its fragments to the uttermost parts of the universe. If the moon were electrified to a like potential, the force of repulsion would be greater than the gravitation attraction to the earth, and it would fly off through space.

For these reasons I rejected the theory, and now believe that the magnetism of the earth still remains, as before, one of the great mysteries of the universe, toward the solution of which we have not yet made the most distant approach.

In connexion with the theory of the earth's magnetism, I had also framed a theory of the Aurora which may still hold. It is that the earth is electrified, and naturally that the electricity resides for the most part on the exterior of the atmosphere—and that the air-currents thus carry the electricity toward the poles, where the air descending leaves it—and that the condensation so produced is finally relieved by discharge.

The total effect would thus be to cause a difference

of potential between the earth and the upper regions of the air both at the poles and the equator. At the poles the discharge of the aurora takes place in the dry atmosphere. At the equator the electrostatic attraction of the earth for the upper atmospheric layers causes the atmosphere to be in unstable equilibrium. At some spot of least resistance the upper atmosphere rushes toward the earth, moisture is condensed, and a conductor thus formed on which electricity can collect; and so the whole forms a conducting system by which the electric potential of the upper air and the earth become more nearly equal. This is the phenomenon known as the thunderstorm.

Hence, were the earth electrified, the electricity would be carried to the higher latitudes by convection, would there discharge to the earth as an aurora, and passing back to the equator would get to the upper regions as a lightning discharge, once more to go on its unending cycle. I leave the details of this theory to the future.

Baltimore, May 30, 1879.

Appendix.—Since writing the above, Professors Ayrton and Perry's paper has appeared in full; and I am thus able to point out their error more exactly. Their formula at the foot of page 406 is almost the same as mine; but on page 407, in the fourth equation, the exponent of n should be $+\frac{1}{2}$ instead of $-\frac{1}{2}$, which increases their result by about 600,000,000, and makes it practically the same as my own.

Rotterdam, July 13th.

XV. *Notes from the Physical Laboratory of University College, Bristol. By* Prof. Silvanus P. Thompson, *B.A., D.Sc.**

I. *On the Source of Sound in the Bell Telephone.*

The question has been at various times and in sundry places discussed whether the sounds emitted by the Bell telephone, when used as a receiver of currents, are caused by molecular vibrations in the instrument, or are due to vibrations executed by the thin iron disk as a whole. The former

* Read April 26, 1879.

theory appears to have been started by Professor Bell himself* in order to account for the transmission of speech by instruments having very thick iron diaphragms, and by the instruments having no diaphragms at all. This view has been also upheld by the Comte Du Moncel in several communications to the learned societies of France. The other view appears to have been first distinctly put forward by Mr. W. H. Preece, in introducing the telephone to the British Association at Plymouth in 1877†; and it has for its most vigorous supporters M. A. Niaudet‡ and Colonel Navez, the latter of whom has replied more than once to points raised by M. Du Moncel. It is a view which appears also to be supported by the recent experiments of Professor Hughes.

The evidence now to be adduced, though not absolutely conclusive on the point at issue, opens out several fresh points of interest. It consists, in brief, of the results obtained by applying to the field of the telephone the experimental method of studying the so-called lines of force, originally due to Gilbert, and developed by Faraday. The details of the method followed by the present writer are identical with those described in his communication of June 23, 1878, "On Magnetic Figures," &c., and which consists in fixing permanently onto glass plates the figures obtained by iron-filings.

The figures obtained by means of iron-filings were resorted to with the view of ascertaining whether the changes in the magnetic field of the telephone were sufficiently marked to account for the alleged motions executed by the iron diaphragm, or whether they were such as to give any support to the molecular hypothesis.

The first step was to investigate the field of a bar-magnet when one pole was placed near a thin iron diaphragm.

It was known at the outset that a thin plate of magnetic matter might be magnetized in an enormous variety of ways. The magnetism might be distributed on the two faces, or in the manner known as *lamellar*; or, instead, any two points in

* A. Graham Bell, "Researches in Electric Telephony," Journ. Soc. Telegr. Eng. 1878, p. 414, vol. vi.

† Rep. Brit. Assoc. 1877, Plymouth, p. 13, W. H. Preece, C.E., "On the Telephone."

‡ *Téléphones et Phonographes*, p. 92.

the disk might be taken as conjugate poles; or any number of poles might be introduced; or, as in the magnets of M. Duter, the magnetization might be radially distributed, the central portion having one polarity, the other polarity existing all round the circumference. De Haldat showed that a variety of irregular magnetizations might be produced by touching steel plates with the pole of a powerful magnet; and the present author also found analogous effects to be produced by passing currents through steel disks. It therefore became a matter of some interest to determine the character of the magnetization of the telephone-disk.

The figures exhibited two unsuspected features:—first, that when the diaphragm is larger than the end-face of the magnet, and even when it does not touch it, the distribution of the magnetism induced in the diaphragm is partially lamellar in character, partially radial. The central portion is magnetized almost entirely normally to its plane; the exterior portion is radially magnetized—a narrow annular region lying between these, in which the character of the magnetization is mixed. It was further observed that this neutral zone is of greater diameter when a larger magnet is employed, and that it enlarges also as the distance separating the magnet and the diaphragm is increased. It is more strongly marked as a region of separation between the central lamellar portion and the outer radially magnetized portion in the diaphragms of thin material than in those of thick. The position of this neutral annular zone is marked in fig. 1 (p. 101), which is a sectional diagram compiled from the figures produced by filings, by a point of flexure in one of the "lines of force" proceeding from the pole towards the diaphragm. The second feature noticed was that some of the outermost lines of force ran round to the front of the disk, entering it very near its circumference.

The next effect to be studied was that produced by the magnetic inductive action of a current traversing a coil of wire about the pole. For the convenience of obtaining the filing-figures upon glass plates but one turn of wire was employed, passing through holes drilled in the glass, and situated as is the coil in the Bell telephone over the pole of the magnet, the position arbitrarily found the most efficient in the

construction of that instrument. In almost all modern elementary treatises on electromagnetics it is shown that the field of a plane closed circuit is equivalent to that of a lamellar magnetic shell of equal strength, or one which has an equal number of lines of force passing through the area it occupies. The result of passing the current around the pole of the mag-

Fig. 1.

Fig. 2.

Fig. 3.

net will therefore be, so far as the field *in the plane of the coil* is concerned, to increase or diminish respectively the number of lines of force due to the magnet by the number of lines of force due to the closed circuit, according as the direction of its field coincides with, or opposes that of the magnet. But the action is not so simple on the regions of the field outside the

plane of the circuit. The *direction* as well as the *number* of lines of force may be altered, and this in a manner so complex as almost to defy calculation, especially if the mutual induction between this magnetic combination and the adjacent iron disk be taken into account. The figures obtained with filings when the current traversed the circuit in opposite directions (see figs. 2 and 3) show that the lines of force proceeding outwards from the pole were in fact thus altered both in number and in direction, and that, in addition to strengthening or weakening the field, the passage of the current had the effect of apparently thrusting the lines of filings forwards towards the iron disk or backwards from it. Moreover the region separating the two separate distributions of magnetism on the diaphragm was shifted on the passage of the current; being contracted in diameter when the current reinforced the magnetism of the pole, becoming enlarged when it passed in the opposite sense.

Knowing from the experiments of Joule and De la Rive that a portion of iron, when magnetized in a particular direction, grows longer in that direction and shorter in its transverse dimension, let us deduce what the effect will be on the diaphragm of a telephone of these two species of magnetization. If the magnetization were radial, the tendency would undoubtedly be, supposing the disk clamped circumferentially, to thrust the middle point of the disk backwards towards the magnet, and to give it a conical shape. If the magnetization, on the other hand, were lamellar, the tendency would be to make the diaphragm thicker, and to contract it over the area thus magnetized. In the actual case where the magnetization partakes of both characters, the two distributions being separated by a neutral zone, the tendency to each form would exist over the regions respectively affected. But the extent of these regions varies with the varying induction of the currents in the coil. Hence, while the total attraction varies, giving rise to oscillations of the diaphragm as a whole, the neutral annular line will also be continually shifting its position and predisposing the diaphragm to take up new nodal forms of vibration, thereby rendering the timbre corresponding to the complicated undulations of the currents arriving from the transmitter.

The result obtained may be regarded from another point of view. If a slight displacement of the iron disk, though unable to affect to any appreciable extent the strength of the magnetic field as a whole, alters its strength at any one point or in any narrow region, or if, even without altering the average number of lines of force in any part of the field, such a displacement shifts the position of some of the lines of force across a narrow region of the field, it may still exercise a considerable inductive action on a closed coil of wire lying in the region where the amount of shifting is greatest. For, since the induced electromotive force in a closed circuit is not proportional to the strength on an invariable magnetic field in which it lies, nor yet to all changes in its strength, but only to such changes as cause a greater or less number of lines of force to pass through the area within the closed circuit, it is evident that the inductive action will be strongest in coils of wire which lie in the region where there is most change in the direction of the lines of force. We have here the *rationale* of the empirical practice of the constructors of the telephone alluded to above—namely, that of using only a small coil of wire, and winding it upon a narrow bobbin placed upon the extremity of the magnet.

Conversely, the passage of a very feeble current through a coil so placed will produce a greater change in the effective intensity of the magnetic field between the core and the diaphragm than would be produced by the same current traversing a similar coil in any other region of the field; for here it has its greatest power to shift the position of the neutral zone, and to alter the distribution of magnetism in the diaphragm.

It would therefore appear unnecessary to form an hypothesis of molecular vibrations in the disk to account for the emission of sounds by the instrument. Such vibrations do in fact exist; but their existence does not necessarily prove that they play any important part in the production of the sound. And it must be remembered that, so far as the disk is concerned, they take place within the narrow range of the extreme positions possible to the neutral annular zone.

Two further experiments seem to confirm the conclusion derived from the foregoing observations. If a compound diaphragm be used, consisting of concentric annuli of thin iron

fixed to a stretched membrane of paper, or if a small iron disk thus fixed be employed, as in Bell's earliest experiment and in some of the experimental telephones of M. Niaudet, a curious timbre is thereby imported into the voices of speakers, though their enunciation is very distinct. A similar result is found to follow the employment of small thick diaphragms. In each of these cases the disposition favours the lamellar distribution of the magnetism.

If, however, a compound diaphragm be employed, consisting of a number of radial pieces similarly fastened to a stretched membrane, tones are well rendered, but enunciation is not distinct. This result is also obtained when the diaphragm of iron is too large in proportion to its thickness. In these cases the greater part of the magnetism is radially distributed.

Whenever a complete theory of the telephone is framed, these are points which must be taken into account.

II. *On a new Variety of Magnetic Figures.*

De Haldat showed that it was possible to produce magnetic writing upon a steel plate by actually writing with the pointed pole of a powerful magnet, the writing being invisible until fine iron-filings were dusted over the plate. In the Physical Laboratory of University College, Bristol, a small circular saw has been found to afford a plate of suitable thickness and quality to produce good results. The latent characters remained for eight months after being inscribed.

While experimenting with these figures, it occurred to the author to try the effect of leading the current of a powerful battery into the plate and of writing on it with the other pole. This done, fine iron-filings were dusted over the plate; and on gently tapping it the writing became legible immediately. A small thin disk of steel which thus had a current passed through its centre exhibited afterwards a magnetism distributed in small concentric rings.

III. *On Magnetic Figures for Demonstration.*

For the production of magnetic figures filings of wrought iron are usually employed, though cast iron answers fairly. Finely powdered magnetic oxide is recommended by some writers, though it does not appear that its employment is

attended with any great advantage. Professor A. M. Mayer took special pains* to produce filings of even quality from specially prepared Norwegian iron; but he says nothing about the size of filings he found best suited for the purpose. Faraday made the remark† that "large and also fine filings are equally useful in turn."

The author, desiring to obtain figures on a larger scale than usual, for purposes of class demonstration, used a number of small steel needles with success. In the case of thin elongated bodies such as these, the magnetic moment is great as compared with the mass; hence it was to be expected that filaments of fine soft iron wire would also yield a good result. Accordingly he had a quantity of fine iron wire gauze of 32 meshes to the inch cut to fragments. The filaments thus produced were scattered in the usual way by means of a pepper-box with perforated lid. The figures given by these filaments with large magnets possess very well-marked characters, and are decidedly superior to those made with ordinary filings.

IV. *On the Magnetic Behaviour of fixed Iron Filings.*

Having occasion to draw the attention of his students to the property of the lines of magnetic force as being at every point tangential to the position of a small freely-suspended magnet placed above them, the author, placing thus a small magnet over the filings fixed some time previously to glass, and from which the magnet producing them had been removed, noticed that they still retained their magnetic property. It then occurred to him to see whether they still possessed directive force as a whole, and found that they appeared capable of attracting and repelling a lightly suspended needle. A figure produced by a single bar-magnet and fixed permanently to a slip of card by gum was suspended lightly upon a needle-point by means of a glass cap. It set itself in the magnetic meridian, and was found capable of being deflected on the approach of a steel magnet. The fixed magnetic curves are therefore themselves magnets.

V. *Magnetic Figures of three dimensions.*

The writer has several times essayed to produce magnetic

* *Vide* American Journal of Science, 1872.

† 'Experimental Researches,' vol. iii. p. 398.

figures of three dimensions. The difficulty in producing them arises from the weight of the iron filings when unsupported, as they must be when the whole figure does not lie in one horizontal plane. With even the most powerful electromagnets the forms of the curves cannot be actually constructed in iron filings for more than a few millimetres length.

Attempts to float iron filings are also difficult, as there is no transparent liquid nearly approaching the density of iron. The writer has tried heavy paraffins and strong solutions of mercuric iodide in potassic iodide. Better effects were obtained, however, when iron filings were employed which had previously been coated with shellac varnish, and which therefore possessed greater buoyancy. The experiment is curious; but the difficulty of seeing across the forests of lines of filings reduces the observation to one of curiosity only.

Another process attempted consisted in plunging a small magnet into a soft paste of plaster of Paris and iron filings. The plaster shortly hardened; and then sections were cut in various directions. The figures observed, however, were poor; and no observations were made of any additional interest as the result of the attempt.

XVI. *The Distribution of Heat in the Visible Spectrum.* *By* Sir JOHN CONROY, *Bart., M.A.**

IN a paper "On the Distribution of Heat in the Spectrum," originally published in the Philosophical Magazine for August 1872, and since reprinted in a volume of 'Scientific Memoirs,' Dr. J. W. Draper states the theoretical reasons for supposing that all the rays in the spectrum have the same heating effect, in the following words:—"A given series of waves of red light impinging upon an extinguishing surface will produce a definite amount of heat; and a similar series of violet waves should produce the same amount; for though an undulation of the latter may have only half the length of one of the former, and therefore only half its *vis viva*, yet in consequence of the equal velocity of waves of every colour, the impacts or impulses of the violet series will be twice as frequent as those

* Read June 28.

of the red. The same principle applies to any intermediate colour; and hence it follows that every colour ought to have the same heating effect."

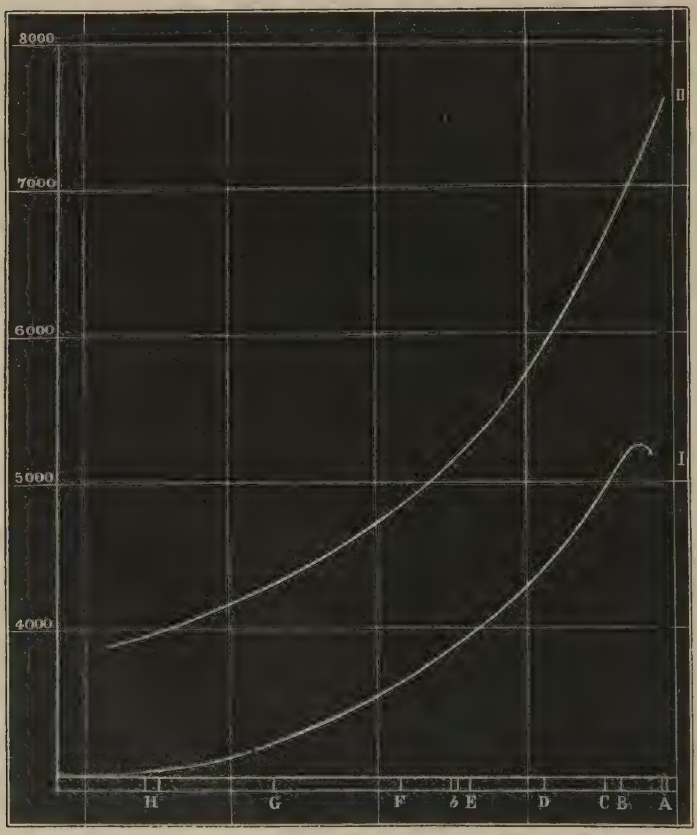
Dr. Draper gives an account of some experiments he has made on the distribution of heat in the visible spectrum of sunlight. He finds that if the visible spectrum between A and H_2 be divided into two equal portions, and all the light of wave-lengths between 7604 and 5768 be collected together, and also all that of wave-lengths between 5768 and 3933, the heat-intensity of these two series of undulations as determined by the thermopile are equal.

The distribution of heat in the spectrum of sunlight had been previously experimentally investigated by Sir W. Herschel (Phil. Trans. 1800, p. 255), J. Müller (Pogg. *Ann.* cv. p. 337), Franz (Pogg. *Ann.* cxv. p. 266), Knoblauch (Pogg. *Ann.* cxx. p. 177, and cxxxvi. p. 66), Fizeau and Foucault (*Comptes Rendus*, xxv. p. 447, and reprinted in the *Annales de Chimie*, 5th series, xv. p. 363), Desains (*Comptes Rendus*, lxvii. p. 297, and lxx. p. 985), Lamansky (Pogg. *Ann.* cxlvi. p. 200). Similar measurements were made with the limelight by Desains (*loc. cit.*) and Lamansky (*loc. cit*), and of the electric light by Professor Tyndall (Phil. Trans. 1866, p. 1). They all found but slight indications of heat in the violet and blue regions of the spectrum, the amount increasing in the green, yellow, and red, and attaining a maximum at a point beyond the end of the visible spectrum.

The experiments were all made by placing a thermometer (one of the ordinary construction being used by Sir W. Herschel and MM. Fizeau and Foucault, and a thermopile and galvanometer by the other observers) in various parts of the dispersion-spectrum formed by prisms of either glass, rock-salt, or sylvine. As Dr. Draper points out in the paper already referred to, this method appears to be an essentially defective one, as, owing to the unequal dispersion by the prism of rays of different refrangibility, a greater number of undulations of different wave-lengths must have been incident upon the surface of the thermometer when it was placed in the red and yellow portions of the spectrum than when placed in the green, blue, or violet portions; and the amount of heat indicated by the instrument being in proportion to the amount of radiant

energy incident upon its surface, the unequal dispersion of the prism would be sufficient to account for some difference in the heating effects produced by different portions of the spectrum.

A graphical method appearing to afford the readiest means of determining the probable effect produced by the unequal dispersion of the prism, a tracing was made, on paper divided into squares of $\frac{1}{10}$ inch, of the curve representing the intensity of the heat in different portions of the visible spectrum, as determined by MM. Fizeau and Foucault (*Ann. de Chim.* 5 sér. xv. p. 377)—the position of the fixed lines in the spectrum, as given by them, being marked on one edge of the paper, which was taken as the x axis, and a scale of wave-lengths in "tenth-



metres" laid down at right angles to this, and the curve for the dispersion of the prism constructed in the ordinary manner.

At nineteen equidistant points in the spectrum the ordinates of the dispersion-curve were measured in wave-lengths; the difference between any two of them gave, approximately, the dispersion of the prism for that portion of the spectrum. A difference of wave-length of 100 was taken as the unit, and the difference between the values of the ordinates divided by 100 considered as a measure of the dispersion. The ordinates of the heat-curve of MM. Fizeau and Foucault at eighteen points in the spectrum, midway between those at which the ordinates of the dispersion-curve had been taken, were measured in tenths of an inch, these numbers divided by those representing the dispersive power of the prism, and the quotients taken as giving the true relative intensity of the heat at the different points of the spectrum.

Ordinates of the dispersion-curve.	Difference divided by 100 (α).	Ordinates of the heat-curve (β).	$\frac{\beta}{\alpha}$.
7260			
6790	4·70	22·4	4·7—B.
6320	4·70	19·0	4·0—C.
6000	3·20	15·9	5·0
5750	2·50	13·7	5·5—D.
5500	2·50	11·8	4·7
5260	2·40	10·3	4·3—E.
5070	1·90	8·8	4·6—*b*.
4900	1·70	7·5	4·4
4750	1·50	6·2	4·1—F.
4610	1·40	5·0	3·5
4490	1·20	4·0	3·3
4380	1·10	3·3	3·0
4270	1·10	2·5	2·2—G.
4170	1·00	1·8	1·8
4070	1·00	1·3	1·3
4000	·70	1·0	1·4
3930	·70	·7	1·0—H.
3880	·50	·5	1·0

The Table shows that the numbers thus obtained for the intensity of the heat in different portions of the spectrum lie close together for the region between B and F, the maximum being near D, and that from F to H the intensity diminishes.

It would further appear that the curve for the distribution of heat of MM. Fizeau and Foucault is in reality a dispersion-curve, drawn to some scale of wave-lengths, for the particular prism used by them; and the diagram shows how very similar the curves for the intensity of the heat and for the dispersion of the prism are to one another.

The heat-curve reaches the x axis at a short distance on the

more refrangible side of H_2, the wave-length of that line being 3932. The axis was taken as 3900 on the scale of wave-lengths to which the curve is drawn, and therefore the ordinate of the curve at B, measured in tenths of an inch, as proportional to the difference between this number and the wave-length of B, and the height of the ordinates at the other lines calculated on this assumption.

	Measured.	Calculated.	Difference.
B	2·10		
C	1·87	1·88	+·01
D	1·34	1·41	·07
E	·95	·97	·02
b	·88	·90	·02
F	·63	·68	·05
G	·22	·29	·07
		Mean difference ...	+·04

Considering the nature of the data, and especially the small scale of MM. Fizeau and Foucault's diagram (the portion representing the visible spectrum being only about 4 inches long), the measured and calculated numbers agree fairly well together.

The same process was applied to the curves given by Lamansky (Pogg. *Ann.* cxlvi. p. 200) for the distribution of heat in the solar spectrum, with flint-glass and rock-salt prisms.

Flint-glass prism.

Ordinates of the heat-curve (β).	$\frac{\beta}{\alpha}$	
22·8	8·4	—D.
21·0	9·5	
19·0	11·8	—E.
17·5	19·4	
16·0	20·9	—*b*.
14·5	20·7	
12·7	15·8	
11·2	14·0	
10·3	14·7	—F.
9·8	14·0	
9·3	13·2	
8·5	12·1	
7·6	10·8	
6·8	8·5	
6·0	7·5	
5·0	7·1	—G.

Rock-salt prism.

Ordinates of the heat-curve (β).	$\frac{\beta}{\alpha}$.	
18·2	6·0	—D.
16·4	6·0	
14·3	7·9	
13·0	9·2	—E.
11·5	8·2	
10·3	7·3	
9·3	7·1	—F.
8·2	5·8	
7·0	5·0	
6·2	4·4	—G.

Assuming the curves to be the dispersion-curves for the prisms, the ordinates were measured and calculated as in the former case.

Flint-glass prism.			Rock-salt prism.		
	Measured.	Calculated.		Measured.	Calculated.
D	23·6		D	18·3	
E	17·8	16·0	E	13·7	13·4
b	15·4	14·6	F	9·8	10·3
F	10·2	10·8	G	6·0	
G	4·0				

In none of the other measurements that have been made of the heat-spectra, as far as I am aware, are the positions of the solar lines stated; and therefore part of the data for eliminating the action of the unequal dispersion of the prisms is wanting.

An attempt was made to deal with Knoblauch's measurements of the solar spectrum, and with Tyndall's of that of the electric light, by the same method.

In both cases the experiments were made with rock-salt prisms; and these were assumed to have the same dispersive power as the one used by Lamansky, and the curve plotted accordingly. Two sets of measurements are given by Knoblauch; and the mean of these was taken for the following calculations.

Knoblauch's measurements.		Tyndall's measurements.	
Ordinates of heat-curve (β).	$\frac{\beta}{\alpha}$.	Ordinates of heat-curve (β).	$\frac{\beta}{\alpha}$.
16·1	2·3	28·3	3·0
12·0	3·6—D.	21·0	3·0
8·7	3·9	15·7	4·7—D.
7·5	4·1	12·0	5·4
7·0	4·6—E.	8·2	5·7
6·8	4·8	6·5	4·3—E.
6·6	4·7	5·0	3·5
6·3	4·5—F.	3·5	2·5
6·0	4·6	2·0	1·7—F.
5·7	4·0	1·5	1·1
		·9	·6

The nature of the available data is such that the only definite conclusions which it is possible to draw from these calculations are, that the distribution of heat in the normal spectrum

differs greatly from that in the dispersion-spectrum, and that in the dispersion-spectrum the great calorific intensity of the red rays, and therefore in all probability of the invisible rays beyond them, is due to the action of the prism in concentrating these rays upon the face of the thermopile. The intensity of the heat in the different portions of the normal spectrum, except in the case of Lamansky's measurements with the flint-glass prism, apparently varies but little through a considerable space; and this affords some support to Dr. Draper's hypothesis, that every colour ought to have the same heating effect.

After I had finished these calculations, I found that G. Lundquist had investigated (Pogg. *Ann.* clv. p. 146), from Lamansky's measurements, the distribution of heat in the normal spectrum, and had shown that it differed greatly from the distribution in the dispersion-spectrum—the maximum intensity in the case of the flint-glass prism being near D, and in the rock-salt prism near E. He also found from Tyndall's measurements of the heat of the electric-light spectrum, that in the normal spectrum the maximum was near A.

Lundquist arrived at these results by a mathematical process, based on the same general principles as the graphical one I have employed.

XVII. *Measuring Polariscopes.*

By Professor W. Grylls Adams, *M.A., F.R.S.**

[Plate XIV.]

Some four years ago the description of a new measuring polariscope was communicated by the author to the Physical Society (see 'Proceedings,' vol. i. p. 152), in which the advantages gained are an extensive field of view combined with accurate means of measuring the rings and the separation of the optic axes in biaxal crystals. The peculiarity of the instrument consists in the arrangement of the two central lenses, one on each side of the crystal. These two lenses are plano-convex, very nearly hemispheres, and, with their flat surfaces inwards, form the two sides of a box to hold the crystal immersed in oil or a liquid; they are so placed that their convex

* Read June 28.

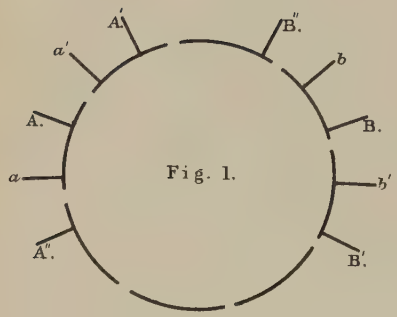


Fig. 1.

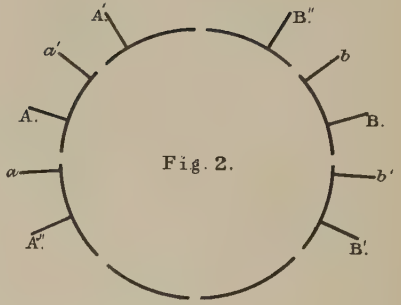


Fig. 2.

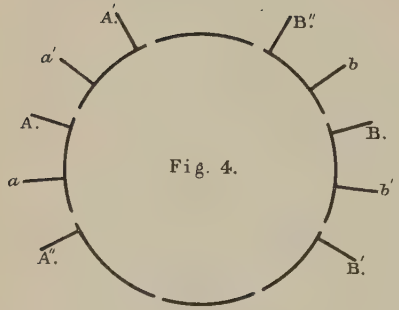


Fig. 4.

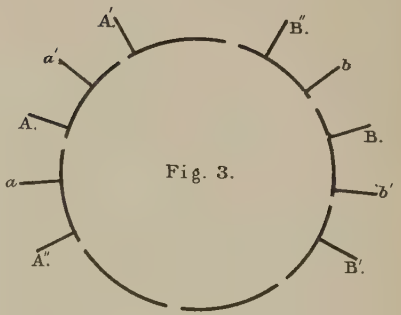


Fig. 3.

surfaces form portions of the same spherical surface. The crystal is placed in the box at the centre of curvature of the spherical surfaces of the two lenses.

Two instruments have since been made on this principle with certain important modifications. In one, made by Mr. Tisley for horizontal projection (Plate XIV. fig. 1), the polarizer is a Nicol's prism capable of giving a clear parallel beam of polarized light $2\frac{1}{2}$ inches in diameter; the middle portion of the instrument (*i. e.* the box with the two equal central lenses for its two opposite sides) has an opening at the top, into which the crystal to be measured is inserted, and is adjusted to its right position by a cup-and-socket motion. When the angle between the optic axes is to be measured, the instrument is placed with its axis horizontal, the crystal is placed with the plane of the optic axis vertical, the box and crystal together are then turned about a horizontal axis at right angles to the direction of the axis of the instrument, *i. e.* at right angles to the plane of the optic axes: thus either of the optic axes of the crystal may be made to coincide with the centre of the field of view, where the spider-lines cross one another, the angle through which the box is turned being measured to minutes by means of a circle attached to it and a vernier attached to the fixed stand supporting the instrument.

A table-polariscope on the same principle has been made by Herr Schneider of Vienna, into which several important modifications have been introduced. A section and view of the instrument are shown in figs. 2 and 3.

The light falls on a plane mirror A, and is reflected into the instrument (which is placed with its axis vertical) through the first lens B, which is fixed on the tube in which the polarizer C is placed.

D, E, F, G, and H (fig. 2) are lenses through which the light passes; and K is a Nicol's prism (the analyzer). This part of the instrument forms a complete table-polariscope of considerable range. The Nicol's prism C and the lenses are supported each by means of two screws (shown in fig. 3), which may be moved upwards or downwards in two slots, and the lenses fixed in their proper positions. Between the plano-convex lenses E and F are the two central lenses L, M, two portions of a sphere.

Fig. 1.

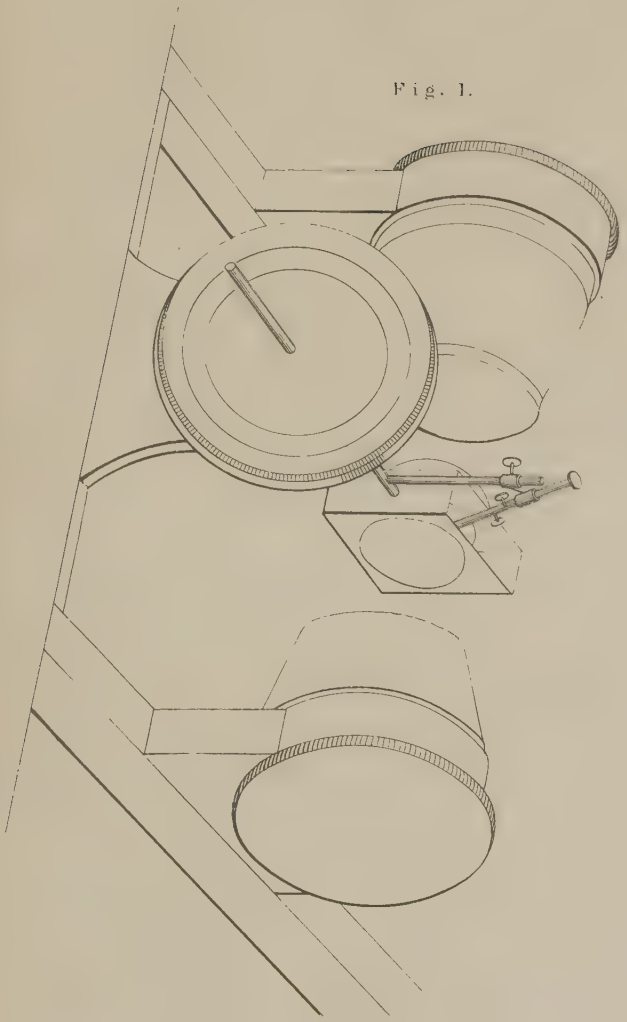
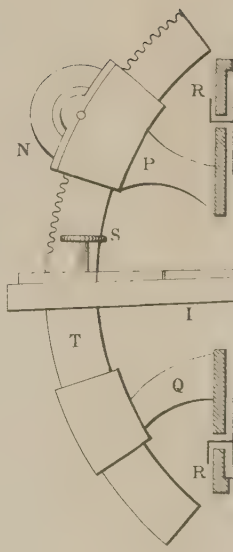


Fig.



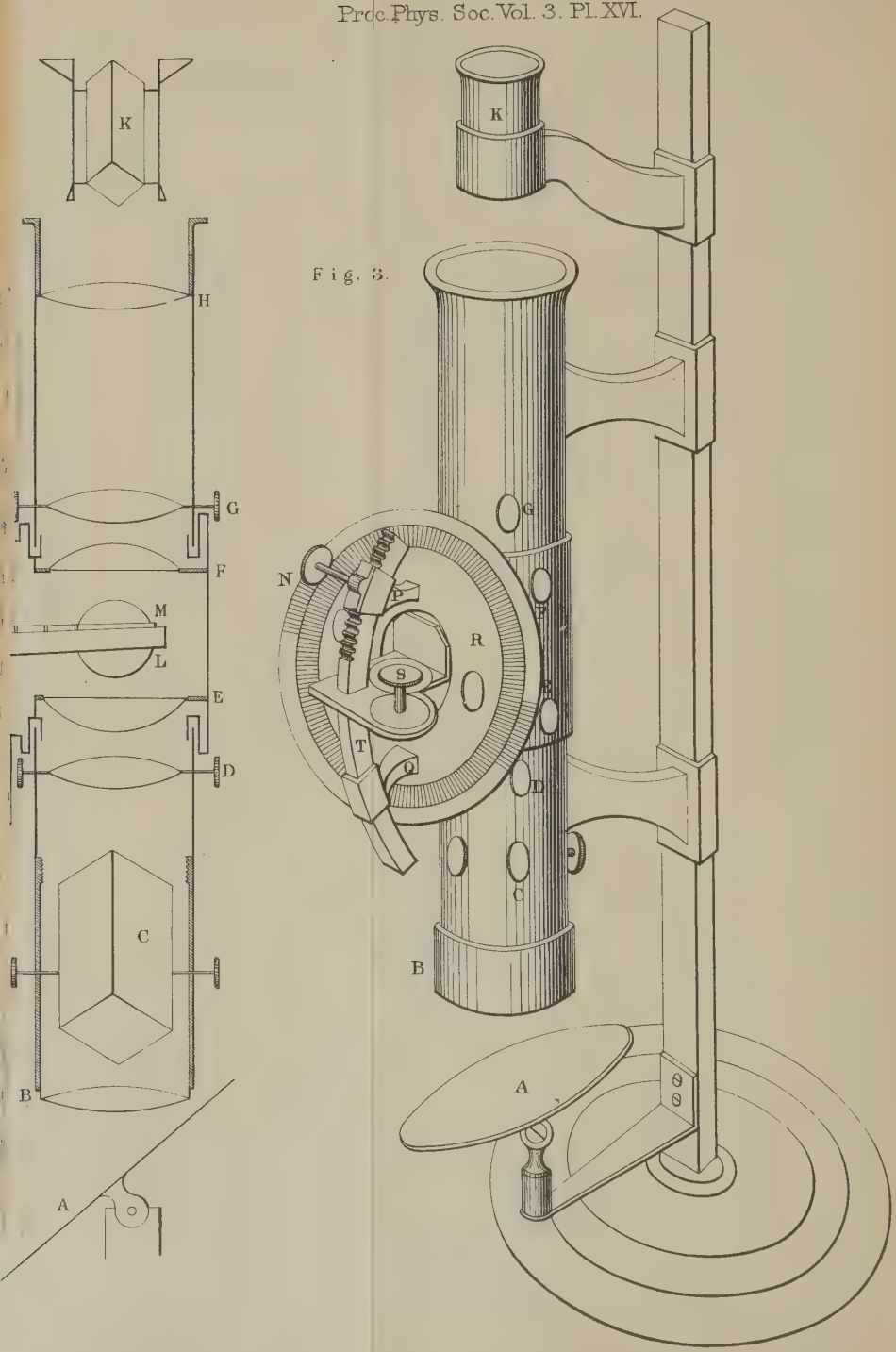


Fig. 3.

IV. By turning the graduated circle with the arc T and the arm I attached to it on the face of the fixed vertical circle (*i. e.* round a horizontal axis perpendicular to the plane of the optic axes), and reading the verniers on both sides of the circle, the angles between the directions of the optic axes may be accurately measured.

XVIII. *A Mode of producing* Arago's *Rotation.*

By Walter Baily, *M.A.**

[Plates XV. & XVI.]

Arago's method of producing rotation in a copper disk consists of suspending it by its centre so as to make it lie horizontally above the poles of a horseshoe magnet, and then rotating the magnet about a vertical axis. The rotation of the disk is due to that of the magnetic field in which it is suspended; and we should expect that if a similar motion of the field could be produced by any other means, the result would be a similar motion of the disk.

Possibly the rotation of the magnet may be the only practicable way of producing a uniform rotation of the field ; but it will be shown in this paper that the disk can be made to rotate by an intermittent rotation of the field effected by means of electromagnets.

Suppose two magnetic poles to be below a plane sheet of a conducting substance capable of moving in its own plane. Each pole may be regarded as a small circular current parallel to the disk. The currents will be in the same or different directions according as the poles are of the same or different names. We will examine the effect of a change in the strength of either of the poles, in giving the sheet a tendency to move.

There are four cases ; viz.—

1. Poles alike. One increasing.
2. „ „ One diminishing.
3. Poles unlike. One increasing.
4. „ „ One diminishing.

In case (1) the increasing pole induces in the portion of the sheet opposite to itself a circular current opposite in direction

* Read June 28.

to the currents representing the poles. Hence this portion of the sheet is repelled by both poles. The repulsion from the increasing pole is perpendicular to the sheet, and gives it no tendency to move; but the repulsion from the other pole tends to move the sheet from the constant pole toward the one which is increasing.

In case (2) the diminishing pole induces in the portion of the sheet opposite to it a circular current in the same direction as those representing the poles; so that this portion of the disk is attracted by both poles, and the disk tends to move from the diminishing pole toward the one which is constant.

In cases (3) and (4) it can easily be shown that the results are the opposite to those obtained in cases (1) and (2) respectively.

If one pole increases while the other diminishes, both tendencies to move are in the same direction, and the resulting tendency is the sum of the two.

The pole of an electromagnet made or broken is the extreme case of a pole increasing or diminishing.

Now conceive an even number of vertical bar electromagnets arranged in a circle with their upper poles in one horizontal plane, and a copper disk to be suspended above them; and the two magnets at the extremities of each diameter to be coupled together, and with a battery, so that each such pair of magnets forms a horseshoe electromagnet independent of all the others. Let P, Q, R, S be pairs of magnets at the ends of successive diameters. Make P, and then make Q so that the north pole of Q is adjacent to the north pole of P, and therefore the south pole of Q adjacent to the south pole of P. Then by case (1) the portion of the disk opposite the north pole of P is driven towards the north pole of Q; and a similar action takes place at the south poles. Now break P. By case (2) the portion opposite the north pole of P is again driven towards the north pole of Q, and so with the south poles. Continuing the action by making R and then breaking Q, making S and then breaking R, and so on, in each case making the adjacent poles similar, we get a series of impulses on the disk all tending to make it move in one direction round the axis of suspension. Hence the disk will rotate as in Arago's experiment.

In one extreme case, viz. when the number of electromagnets is infinite, we have the case of a uniform rotation of the magnetic field, such as we obtain by rotating permanent magnets.

In the other extreme case the number of pairs of electromagnets is reduced to two, and the number of batteries is also reduced to two. Let the poles of one pair of magnets be called a, a', and those of the other b, b'. Then the arrangement of the pole as seen from above is $\begin{matrix} a & b \\ b' & a' \end{matrix}$; and the successive states of these poles will be

1.	2.	3.	4.	5.	6.	7.	8.	1.
NN	ON	SN	SO	SS	OS	NS	NO	NN
SS	SO	SN	ON	NN	NO	NS	OS	SS

N, S, representing north and south poles respectively, and O representing that the pole is not magnetic. It appears that the change from 1 to 3 through 2 is nothing more than reversing $a\,a'$; and the change from 3 to 5 consists in reversing $b\,b'$. Similarly we pass from 5 to 7 by reversing $a\,a'$, and again from 7 to 1 by reversing $b\,b'$. The whole process is thus shown to consist in reversing $a\,a'$ and $b\,b'$ alternately.

In passing from 8 to 1 we see by case (1) that the parts of the disk over b, b' are respectively repelled from a, a'; and by case (3) we see that the same parts are respectively attracted to a', a. Again in passing from 1 to 2 we see by case (2) that the parts above a, a' are respectively attracted by b, b'; and we also see by case (4) that the same parts are respectively repelled by $b'\,b$.

The effect of each of these two changes is to make the disk tend to rotate in the direction $a\,b\,a'\,b'$. All the other changes may be shown to have the same effect; so that the disk will rotate in the direction $a\,b\,a'\,b'$.

If starting with the state (1) we reverse $b\,b'$ first, we should have the series of states as follows:—

1.	2.	3.	4.	5.	6.	7.	8.	1.
NN	NO	NS	OS	SS	SO	SN	ON	NN
SS	OS	NS	NO	NN	ON	SN	SO	SS

from which it may be easily shown that the disk would rotate in the direction $b\,a\,b'\,a'$.

The experiment with the four electromagnets may be readily performed by means of a commutator which will reverse the currents several times in a second; and a considerable rotation can be given to the disk.

The commutator which I constructed for the experiment consisted of a wheel of wood with a brass rim. This rim was completely cut through in places equidistant from each other; and ten tongues of thin copper pressed against the rim*. These tongues were in two groups of five; and in each group the distances between the contacts of the tongues with the rim were half the distances between the cuts in the rim. The distance between the two groups must be greater than the distance between the contacts.

Let A be a wire from the positive pole of one battery, and A′ A″ be wires from the negative pole of the same battery; B a wire from the positive pole of the other battery, and B′ B″ wires from the negative pole; and let $a\,a'$ be the ends of the coil round one pair of electromagnets, and $b\,b'$ the ends of the coil round the other pair. In the figures 1 to 4 the wheel is seen in four consecutive positions, with the tongues in contact with it; and the letters show with what wire each tongue is connected.

The contact of two tongues with the same section of the ring puts the wires to them into electric connexion.

The connexions are, in

Fig. 1.	A a,	A′ a',	B b,	B′ b',
Fig. 2.	A a',	A″ a,	B b,	B′ b',
Fig. 3.	A a',	A″ a,	B b',	B″ b
Fig. 4.	A a,	A′ a',	B b',	B″ b
Fig. 1.	A a,	A′ a',	B b,	B′ b'.

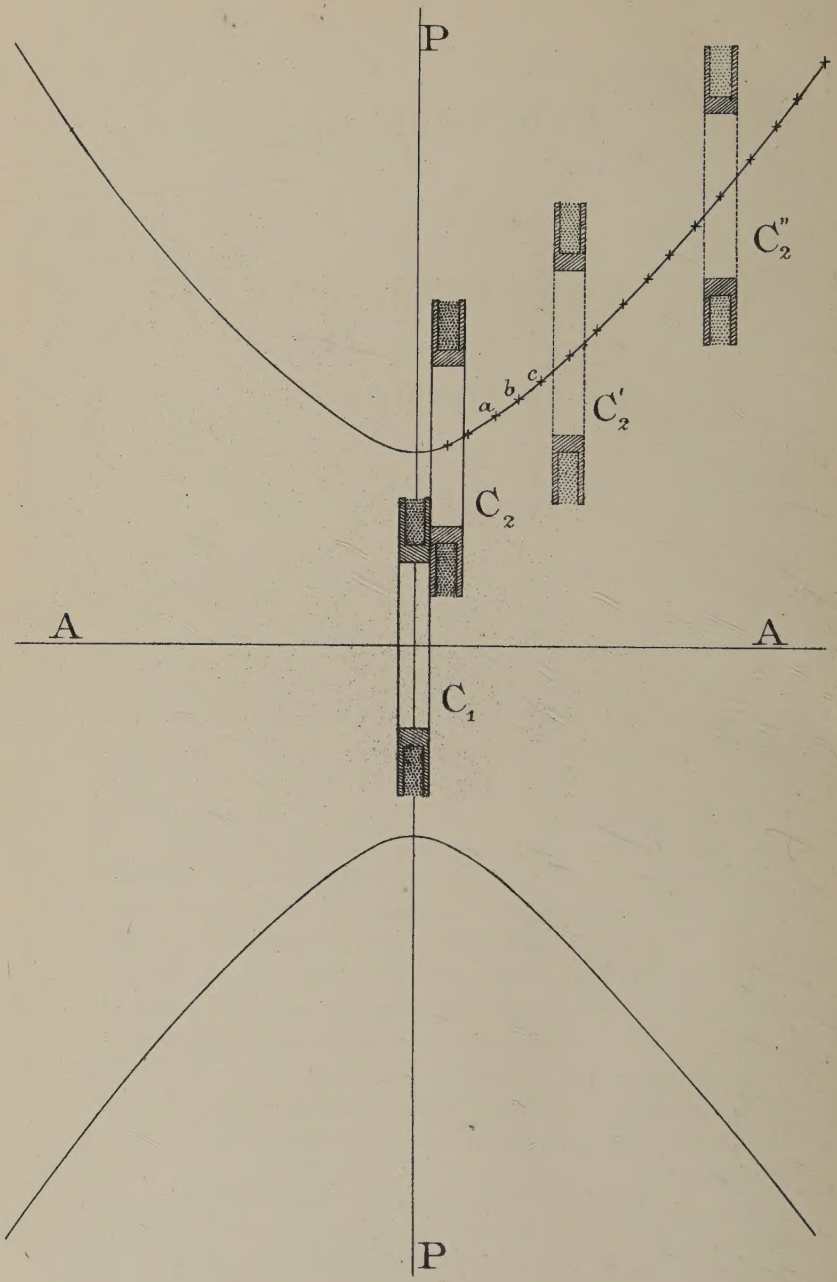
Hence in passing from fig. 1 to fig. 2 the current through $a\,a'$ is reversed; in passing to fig. 3 that through $b\,b'$ is reversed. The current through $a\,a'$ is reversed again in passing to fig. 4, and that through $b\,b'$ is reversed again in passing to fig. 1. The commutator is thus seen to reverse each pair of magnets twice while rotating through the angle subtended by one division of the rim; so that with eight divisions one turn of the wheel reverses each pair of magnets sixteen times.

* The number of cuts must not be less than seven. I used eight cuts.

If the wheel is rotated in the opposite direction, the series of magnetic states is obtained in the reverse order, and the disk rotates in the opposite direction. But there is a better method of reversing the motion of the disk—which is, to introduce an ordinary commutator into one of the circuits, either between the battery and the wheel, or between the wheel and the magnets. The reversal of this commutator reverses the motion, while the wheel is rotated continuously in one direction. Fig. 5 gives the arrangement of the connexions. A, B are the batteries; a, a', b, b' the electromagnets; C the ordinary commutator; and D the wheel.

It should be noted that the rotation of the disk is accompanied by the formation of induced currents whose intensity depends on the velocity of rotation, and whose effect is to diminish the rotation. These opposing currents are got rid of when the disk is suspended as a torsion-balance, and its deflection observed, as the currents will not be formed except when the disk is moving.

The effect on the disk might be much increased by placing four other electromagnets above the disk, each opposite one of the lower magnets, as connected with it, so that the lower pole of the upper magnet should be of the opposite name to the upper pole of the lower magnet. In fig. 6 one pair of magnets is shown with the opposite pair, and the wires connecting them. The disk is seen in section, balanced on a needle-point, between the two pairs of magnets. The other four magnets are not shown in the figure.


P
C"2
a
b
c
C'2
C2
A
A
C1
P